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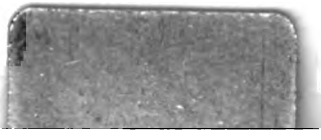

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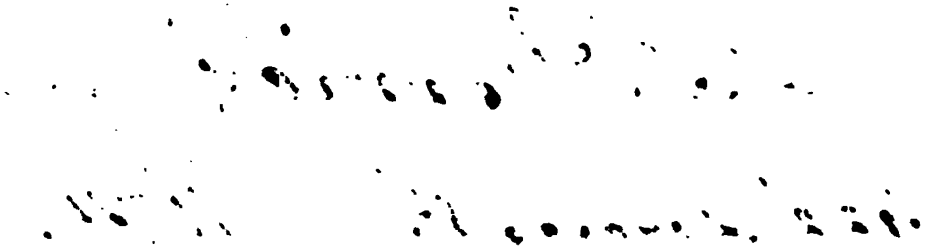

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MEDICAL NOTES AND REFLECTIONS.

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MEDICAL

NOTES AND REFLECTIONS.

BY

SIR HENRY HOLLAND, BART., M.D., F.R.S.,
ETC. ETC.,
FELLOW OF THE ROYAL COLLEGE OF PHYSICIANS,
PHYSICIAN IN ORDINARY TO THE QUEEN, AND PHYSICIAN IN ORDINARY TO
HIS ROYAL HIGHNESS PRINCE ALBERT.

Φαινεται ουτε περας, ουτε τελευτην εχων· ὁτι προ τε της αρχης, αλλη αει φαινεται αρχη· μετα τε την τελευτην, ἑτερα ὑπολειπομενη τελευτη.—PLATO (*de Scientia*).

FROM THE THIRD LONDON EDITION.

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PREFACE

TO THE AMERICAN EDITION.

THE first Edition of this work was republished at Philadelphia in 1839; and so received by my professional brethren in America as to justify, I hope, the suggestion, now made to me by some of them, of a republication of this third English Edition, in its altered and enlarged form. Three successive visits to the United States have given me sufficient knowledge of the eminent character of the medical men of that country, and of the high social position they hold, to render me earnestly desirous of gaining a name and good opinion amongst them. And this desire is strengthened by the kindness and courtesy I have ever experienced at their hands. To the suggestion made to me therefore I gladly assent.

I may venture to speak of this work as having been successful in England; and many circumstances, beyond the mere demand for a third Edition, give me reason to hope and believe that it has been one of practical usefulness to the profession here. If it should serve to any similar good in its present form and place of publication, such result will on every account be welcome to me.

The Preface to this third Edition will show the nature and extent of the changes which have been made in the work, and the reasons which have led to them.

HENRY HOLLAND.

LONDON, October 31, 1856.

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PREFACE

TO THE THIRD EDITION.

A THIRD Edition of this work having been called for, it is necessary to preface the volume by a few remarks, explanatory of certain alterations which have been made in its contents; both as respects the several topics treated of, and the manner of their present arrangement.

In the two former Editions, a few chapters were included on psychological subjects—the mutual actions and relations of mental and bodily phenomena, which constitute the life of man. Closely blended though these topics are with every part of physiology and medical practice, they yet involve peculiar methods of discussion, which separate them in some sort from the other parts of the work. This consideration led me, three years ago, to detach these particular chapters, and to embody them in another volume, under the title of "Chapters on Mental Physiology;"—enlarging them generally, and adding others, which might give more unity and completeness to the treatment of the subject. The removal of these chapters has led me to supply their place by a few others, taken from among the papers I had originally designed for a second volume of this work. The parts thus added have more explicit relation to practical medicine, and come therefore into closer accordance with the other topics treated of in the volume. In the prior editions, from causes stated in the preface, the arrangement of the subjects was desultory throughout; a fault, doubtless, in itself, yet one which the nature of my plan and materials scarcely permitted me to avoid. In the present Edition I have sought (aided by the change just mentioned), to bring the chapters more into series

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as regards the relation of their subjects; and, I trust, not without some advantage to the reader.

In the earlier parts of the volume, they will be found to relate chiefly to certain pathological principles, through which I have sought to associate together various morbid conditions, not usually thus connected in our systems of nosology. This method of inquiry, which I believe to conduct to many valuable results, I have further carried into the consideration of some particular disorders (Gout, Influenza, Dyspepsia, &c.), which at once illustrate the method in question, and are themselves best illustrated through it. The chapters comprised in the latter half of the volume relate more especially to the treatment of disease; including under this term not solely the consideration of particular medicines or remedial means; but also the methods in practice which a physician may expediently follow, and the duties he is bound to fulfil.

Sixteen years have now been added to a period of practice, already long when the first edition of this work was published. In these times of rapid and incessant change—of medicine as of other sciences and professions—such a period becomes a little history in itself; and no one of common observation can have gone through it without gathering up, not merely insulated cases and facts, but also many general conclusions and principles, of still greater concernment to the interests of the profession. Such knowledge, as derived from my own experience, it has been my endeavor to convey in the clearest and most practical form in the following chapters; which must be regarded, however, not as formal treatises on the several topics discussed in them; but simply as selections from what I have noted in practice; embodying in this form some out of the numerous materials which are yet required for future and more complete research.

The chapter on "Animalcule Life as a Cause of Disease," illustrated by the history of Cholera, I have considerably enlarged; placing it, however, at the end of the volume, as being in some measure detached by its more hypothetical character from the other subjects of which I have treated.

London, August 18, 1855.

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PREFACE

TO THE FIRST EDITION.

THE title of this volume is chosen as being that which most nearly expresses its contents. Though appearing now as detached papers, they are founded chiefly upon notes made in the course of twenty years of medical practice in London. During nearly the whole of this time, I have been accustomed to preserve notices, not merely of particular cases, but also of such general reflections as were suggested to me by actual observation. At the expiration of the period named, I have thought it well to look back upon these various memoranda; to give something of more definite form to those which seemed worth preserving; and to compare the whole, as well with my own present impressions, as with the actual state of knowledge on the several subjects in question. This volume is the result of a revision and selection so made. But as its form, though sanctioned by precedents of high value, is not altogether common, it may be right to add a few words more, in explanation of the motive and manner of publication.

It seemed to me, looking at them as impartially as I was able to do, that there were among these papers so revised, a certain number which might be likely to contribute in some degree to medical knowledge or to the exactness of our views in practice. I have had regard to these objects only, as justifying publication; and as a principle of selection out of the materials before me. In making this selection, I have put aside a great deal as relating to subjects of inferior importance; still more, from finding that many of my notes related to facts or opinions no longer new.

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What I have retained includes much that will be familiar to all who have carried observation and study into their medical life. But this was inevitable, without wholly omitting many subjects of great interest; and it would have been presumptuous to offer a work composed, as this has been, from materials acquired in the course of active practice, as one of original research.

Every physician has, in his progress, some particular occasions or facilities capable of being converted to good. The opportunities of private practice, from which almost exclusively my own observations are drawn, do not furnish the same striking conclusions as those of hospitals; nor the larger classes of facts, which form the statistics of medicine, and are so fruitful of results. Yet such is the scope of the subject, that a prolonged experience, with due regard to the nature and sufficiency of the evidence, may from this more limited source derive much to aid other methods of research, and to enlarge the general amount of medical knowledge.

What I would fain hope may be found in this volume is, a just view of some of the relations of diseases, as well to each other as to the healthy functions of the body;—the correction of some doubtful or erroneous views in practice;—suggestions which may be useful as regards particular classes of remedies;—and reflections on certain points of physiology, in which without any pretension to experimental inquiry, it appeared to me that something might be gained by arranging the facts and inferences in a new form. On topics of the latter class I have sought especially to associate pathology with physiology, the morbid with the natural and healthy states of the body; believing this principle of modern inquiry to be above all others, fertile in sound conclusions, and far from being yet worked out to its full extent. If I might venture on giving any distinctive character to the volume, it would be that of aiming throughout at this object.

In one instance only have I indulged in any mere speculation; and this only interrogatively, as to an old hypothesis, regarded in its relation to modern science, and to the history of a remarkable disease. Many other points I have put as questions; finding them as such in my notes; and thinking it well, when-

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ever it could be done, thus to mark the objects most open to inquiry. I have further taken the privilege (which experience may perhaps be allowed to sanction) of commenting on certain usages and details of practice, in which the character and usefulness of the profession, and through them, the welfare of the public, are materially concerned.

On each subject treated of, I have brought together my notes in the manner best suited for perspicuity; adding whatever seemed necessary to give greater completeness to the reasoning, or to connect it with the inquiries of others on the same topics.[1] In effect of this, much of the volume has been wholly written anew. I had at one time proposed the insertion of a greater number of the cases upon which its materials were founded; but I abandoned the intention, from a wish not to increase the size of the work, which this must largely have done. And for the same reason I have abridged into the form of notes many topics on which I had originally written at greater length.

As respects the arrangement of the subjects, it will be found a very desultory one; such as naturally arose out of the miscellaneous materials employed. In a few instances only have I thought it worth while to bring topics expressly together for their mutual elucidation. To have attempted this further would imply a more complete and consecutive work than that now offered to the public.

These, I think, are all the circumstances which need be stated in explanation of the form and matter of the volume. It will deeply gratify me, if hereafter I find cause to believe that it has contributed to science by any right views in the philosophy of medicine, or to practical good by any suggestions of value regarding the treatment of disease.

[1] On the latter point great deficiencies are inevitable. In these days of various research, actively pursued in so many countries, it would require more reading than is compatible with actual practice to collect together all that has been done on these subjects; nor would a single volume suffice for the mere references needful to such a work.

LOWER BROOK STREET, March 13, 1839.

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MEDICAL

NOTES AND REFLECTIONS.

CHAPTER I.

MEDICAL EVIDENCE.

THERE can be few better tests of a sound understanding, than the right estimation of medical evidence; so various are the complexities it presents, so numerous the sources of error. The subjects of observation are those in which Matter and Mind are concurrently concerned;—Matter under the complex and subtle organization, whence vitality and all its functions are derived; Mind in its equally mysterious relations to the organs thus formed; —both subject to numerous influences from without, both undergoing very various changes from disease within. Individualities of each have their effect in creating difficulties, and these amongst the most arduous which beset the path of the physician.[1] Few cases occur strictly alike, even when the source of disorder is manifestly the same. Primary causes of disease are often wholly obscured by those of secondary kind. Organs remote from each other by place and function are simultaneously disturbed. Translations of morbid action take place from one part to another. Nervous affections and sympathies often assume every character of real disease. While remedial agents are rendered uncertain in effect by the various forms of each disorder, by the idiosyn-

[1] Idiosyncrasy, as arising in most cases from inappreciable causes, is the most absolute and inevitable difficulty in medical evidence; since no accumulation of instances, such as might suffice for the removal of all other doubts, can secure us wholly against this source of error.

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crasies of the patient, by the difficulty of securing their equal application or transmission into the system, and finally by the unequal quality of the remedies themselves.

These difficulties, the solution of which gives medicine its highest character as a science, can be adequately conceived by the medical man alone. Neither those accustomed to legal evidence only, nor such as have pursued physical science in its more simple material forms, can rightly apprehend the vast difference made by the introduction of the principle of life; or, yet more, of the states and phenomena of mind, in connection with bodily organization. We have here a new world of relations, occult and complex in their nature, to be reasoned upon and resolved; with a principle of change, moreover, ever operating among them, and deviations from nature under the form of disease, which render all conclusions liable in a thousand ways to error. It is the want of this right understanding of medical evidence, which makes the mass of mankind so prone to be deceived by imposture of every kind; whether it be the idle fashion as to particular remedies; or the worse, because wider, deception of some system, professing to have attained at once, what the most learned and acute observers have labored after for ages in vain.

It must be admitted, indeed, that this matter of medical testimony is too lightly weighed by physicians themselves. Else whence the frequent description of effects and cures by agents put only once or twice upon trial; and the ready or eager belief given by those, who, on other subjects, and even on the closely related questions of physiology, would instantly feel the insufficient nature of the proof.[1] Conclusions requiring for their authority a long average of cases, carefully selected, and freed from the many chances of error or ambiguity, are often promulgated and received upon grounds barely sufficient to warrant a repetition of the trials which first suggested them. No science, unhappily, has abounded more in false statements and partial inferences; each usurping a place for the time in popular esteem; and each sanctioned by credulity, even where most dangerous in application to practice. During the last thirty years, omitting all lesser instances, I have known the rise and decline of six or eight fashions of medical doctrine or treatment; some of them

[1] "Id credunt esse experientiam, quando semel vel bis faustam, vel infelicem, in certo morbo a sumpto medicamento annotarunt efficaciam."—*Hoffman.*

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affecting the name of systems, and all deriving too much support from credulity or other causes, even among medical men themselves.

Look at what is necessary in strict reason, to attest the action and value of a new remedy or method of treatment. The identity or exact relation of the cases in which it is employed;—a right estimate of the habits and temperament, moral as well as physical, of the subjects of experiment;—allowance for the many modifications depending on dose, combination, quality of the medicine, and time of use;—due observation of the indirect or secondary, as well as direct, effects;—and such observation applied, not to one organ or function alone, but to the many which constitute the material of life. All these things, and yet more, are essential to the completeness of the testimony. All can rarely, if ever, be reached; and hence the inevitable imperfections of medicine as a science. But much more, doubtless, of truth and beneficial result might be attained, were these difficulties rightly appreciated, and the fit means of obviating them kept more constantly in view.[1]

In no class of human events is the reasoning of "*post hoc propter hoc*," so commonly applied by the world at large, as in what relates to the symptoms and treatment of disease. In none is this judgment so frequently both erroneous and prejudicial. It would seem as if the very complexity of the conditions necessary to sound evidence, tended to beget aquiescence in that which is lightest and most insufficient for truth. The difficulties occurring in practice from this source are great, and require a right temper as well as understanding to obviate them.

Nor is there any subject upon which words and phrases, whether applied to diseases or remedies, exercise a larger influence. Terms have descended to us, which we can hardly put aside,—maxims, which fetter the understanding and power of action,—and methods of classification, which prevent the better suggestions of a sound experience. And these are among the evils most aggravated by public opinion, ever prone to be governed by names, and particularly in all that concerns the symptoms and treatment of disease. The deeper the interest belonging to the subject, the greater the liability to error.

[1] Amongst the other difficulties of evidence in such cases must be noticed the ambiguity of all language as applied to denote and distinguish sensations; an evil familiar to every medical man, and only to be obviated by watchful experience.

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It needs to be called to the mind of medical men themselves how many organs of the body are still without any certain interpretation of their part in the animal economy. Even that great organ, the Liver, though now known to us in its most minute anatomy, offers many problems for more complete solution;—the conclusions of Liebig and Bernard (themselves recent in date), differing as to the nature of its functions, and their relation to the respiratory organs. The uses of the ductless glands —the spleen, thyroid, thymus, and supra-renal bodies—are yet only vaguely conjectured. The physiology of the brain and nervous system, though largely elucidated by the researches of the last forty years, still abounds in difficulties, many of which time may overcome; while others are probably forever insuperable.

These few instances indicate one great source of error and embarrassment, to which all medical proof is exposed. But in pathological doctrine, also, there is similar and equal difficulty from the character of the evidence concerned. If example were needed to illustrate this, it might be drawn from what relates to the history of Fever, whether idiopathic or symptomatic in kind. Here centuries of research, amidst facts of daily occurrence, have yet left some of the most important questions wholly unresolved;—such is the difficulty of obtaining unequivocal proofs of nature essential to a just theory. The same with respect to the true doctrine of Inflammation; a question which spreads itself, directly or indirectly, over every part of pathology. Here the most various and diligent inquiries long left it uncertain, whether there occurred an increased or diminished action of the capillaries of the inflamed part; or in what mere turgescence differs from inflammation. It is only of late years, that the improved power and refined use of the microscope have given us more assurance as to the nature and succession of these changes; still leaving, however, various points open to future determination.[1]

The history of contagious diseases furnishes another instance not less remarkable. It is the common belief of the world, and

[1] I allude here especially to Kaltenbrenner's microscopic researches. The frequent revival of controversy on these points among modern writers shows at once their importance, and the incompleteness of the knowledge yet obtained on the subject.

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one plausible enough in its first aspect, that the laws of contagion are simple and readily learnt. No mistake can be greater than this. All parts of the subject, even the circumstances most essential to practice, are wrapt up in doubt; and the evidence is of so intricate a kind, and so much disturbed by seeming exceptions, that the best judgments are perpetually at fault upon it. The same remark may be extended to other classes of disease, where time and the most acute observation have hitherto failed to extricate truth from the multitude of conditions present; but where, nevertheless, we have the certainty of relations hitherto undetermined, the fixing of which belongs to future research, and will well repay it in the result.

The observations just made, are not less true in relation to the internal remedies we employ, even those most familiar, and seemingly simplest in use. The difficulties already noticed, as belonging to the evidence of their effects, extend yet further, and more remarkably, to the theory of their action; and our knowledge, though augmented of late years by the wonderful advance of Organic Chemistry and Physiology, can scarcely yet be said to have passed the threshold of the inquiry. We must avow that this whole department of medical knowledge, so important in all its bearings, is the one where most remains to be done to raise it to the character of a science. The real amount of the deficiency can only be understood by a candid examination of the results of experience, and a fair comparison with other branches of physical knowledge.

To many, indeed, even of those conversant with practice, this will seem too disparaging a view of the science they profess. And, in truth, the accustomed way in which the uses of medicines are taught, and the methodical forms under which they are classed and named according to their alleged effects, do favor the persuasion of a more advanced state of knowledge than that just described. It might be enough, perhaps, to refer those who doubt the reality of the view I have taken to what has been written on medicines in general, and to what is unequivocally known of each one in particular. It will be found that ambiguities of language crowd round every part of the subject—that classification, while clearing away some difficulties, has created many more—that evidence, feeble in its origin, has often gained weight by mere transmission from one hand to another—and

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that this matter is peculiarly one where the prejudices and credulity of the world usurp upon the sense of the practitioner, and warp his practical judgment.

If the truth be really as here stated, in regard to our knowledge of internal remedies, it is a feeble and useless policy to disguise what is thus deficient. The avowal may more readily be made; first, because the difficulties to be overcome are greater than in any other science of observation or experiment;—and secondly, because, despite these difficulties, the scope and prospect of future attainment are ample and certain, and the connection such with other branches of physics, that the progress of each is fitted to throw light upon what is here doubtful or obscure. Already it may be affirmed that the recent methods of research in medicine have gained greatly in exactness, and the just appreciation of evidence, upon those of any previous period:—a natural effect of increasing exactness in all other branches of science; and, it must be added, of increasing and well-directed energy amongst those engaged in the profession. The deficiency just stated is greater, perhaps, than any other, and more difficult of redress; but time and various new means and appliances are in our hands, and it would be an injury done to the cause of science to doubt their effects.[1]

A very especial advantage we now possess, consists in the application of numerical methods and averages to the history of disease; thereby giving it the same course and certainty of result which belong to statistical inquiry on other subjects. We do not exceed the right expression of fact, in terming this method of averages one of the most important principles and instruments of philosophic research. Little, if at all, recognized as

[1] Some may think the view here given of the progressive advance of rational medicine to be contradicted by the recent growth of Homœopathic practice in this country. It is true, indeed (apart from what may be called the homœopathic principle), that no case exists of more faulty evidence than that upon which the founder of this system and his followers have catalogued together symptoms (amounting frequently to some hundreds, and derived from parts wholly diverse in structure and function), as produced by inappreciably small doses of substances which are utterly inert when largely augmented in quantity. In another chapter (On Disorders of Digestion) I shall speak of what I believe to have been the main causes of the spread of this practice in England. Depending for its success upon practical abuses still not wholly removed, it may maintain for yet longer time a repute thus acquired; but, like many other fashions of earlier date, must eventually yield to the conclusions of a sounder science, and better principles of evidence.

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such in former times, it has now become a law in itself, and the index and interpreter of truth in every department of human knowledge. Under the guiding genius of Herschel and Struve, it has been applied to the most profound questions in sidereal astronomy; while under a widely different use, it has been brought to the solution of some of the most curious problems of human life and social existence. The vast interval between is replete with examples of its successful application.

Averages may, in some sort, be termed the mathematics of medical science. It is obvious, indeed, that the value of inferences thus obtained depends on the exact estimate of what are the *same facts*—what merely connected by resemblance or partial analogy. Pathological results, essentially different, may be classed together by inexact observers, or by separate observers under different views. These, however, are errors incident to every human pursuit, and best corrected by repeated applications of the method. The principle in question is indeed singularly effectual in obviating the difficulties of evidence already noticed; and the success with which it has been employed, of late, by many eminent observers, affords assurance of the results that may hereafter be expected from this source. Through medical statistics lies the most secure path into the philosophy of medicine.[1]

In looking further to the chance of overcoming these difficulties, in the future, regard must be had to the principle now verified in so many cases, that in proportion to the complexity of phenomena is augmented also the number of relations in which they may be surveyed and made the subjects of experiment.[2] The application of this principle to medical science is every day

[1] The inquiries which so greatly distinguished M. Louis as a pathologist, may be noted as eminent examples of this method, which is now pursued with great success by many physicians in our own country. The materials furnished for it, under the new Registration Act, are of the most valuable kind; and the volumes already published give full proof of what will be effected for medical science by such a system, so admirably conducted. Already numerous results and generalizations have been placed before us, wholly unattainable in any other way; and, on the simple principle of averages, every year tends to enlarge and confirm these conclusions.

[2] M. Comte, in his "Cours de Philosophie Positive," has defined this as a general law, through which compensation is made for the difficulty or impossibility of giving a mathematical form to certain branches of physics—such particularly as relate to the phenomena of organic bodies.

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becoming more apparent. The fine expression of Laplace, "Tout se tient dans la chaine immense des vérités," is well exemplified here. Every new path of physical knowledge opened, each single fact discovered, has given guidance more or less direct towards the objects still unattained in physiology and the treatment of disease. The unexpectedness of some of the relations thus determined and converted to use, is the best augury for further advances in this direction of pursuit.

A due estimate among medical men themselves of the nature of the proofs with which they have to deal is more especially needful at present, when the older doctrines of physic and physiology are all undergoing the revision required by modern science; and when new medicinal agents are every day produced upon trial, many of them dangerously active in their effects, many suggested by analogies which need to be verified by the most cautious experience. Hasty and imprudent belief may here become a cause of serious mischief;—the wider in its spread, as the minds most prone to this credulity are those most ready also to publish to the world their premature conclusions; and thus to mislead the many who found their own practice upon faith in others; or who seek after novelty, as if this were in itself an incontestable good.

And here I must advert to another circumstance which renders strict attention to the laws of evidence a matter of peculiar obligation at the present time. This is the tendency, so marked in modern physiology, to carry its researches into the more abstruse questions connected with vitality, the nervous power, and the relations of mental and material phenomena—inquiries justifiable in themselves, but needing to be fenced round by more than common caution as to testimony, and the conclusions thence derived. Yet here, especially, it is that such precautions have been disregarded;—partly, it may be, from the real difficulty and obscurity of the subject—still more, perhaps, from the incompetency of many of those who have taken it into their hands. For these researches, no longer confined to a few, as heretofore, have become the property and pursuit of many who wander merely on the confines of science, believing they are within its pale; and whose speculations on what they see are little checked by collateral knowledge, or by a due estimate of the laws and limits of scientific inquiry. The mystery of the

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subject is in itself a charm and seduction to the mind. They see, and give attention for the first time, to those wonderful phenomena which, though inherent in the constitution of man, and in the relations of his mental and corporeal nature, are not familiar to common observation. The feelings are thereby excited even more than the reason; and belief is hurried on, and results accredited, with little care for the sufficiency of proof, or knowledge of the many facts which otherwise explain, or contradict, the conclusion. However earnest the desire for truth, imposture rarely fails to mix itself with inquiries so conducted; and increases the practical evil which always more or less results from error. Every philosophical physician is bound to watch over these events as they pass before him; never refusing inquiry, because what is put forward is new or strange; but requiring evidence in proportion to the unusual character of the facts; sifting closely that which is offered, and rejecting all conclusions not founded on this basis.

While speaking on the question of evidence as applied to these subjects, it is impossible not to advert to the fact—a very important one in the history of the human mind—of the great diversities of intellectual constitution as to this point. It is, in truth, one striking expression of the difference of the reasoning power, testified on the most ordinary occasions of life, and very remarkably where the subjects rise to the character of science. Locke has well said, "There are some men of one, some of two syllogisms, and no more; and others that can advance but one step further." The distinction here expressed applies closely to every question of evidence. One man concludes upon proof, which, to another, has neither weight nor pertinency. One mind pursues a subject throughout all its relations; another follows but a single line towards the result. "Some men, in passing through the boundless ocean of disquisition, mistake fogs for land." There are others whom Lord Bacon calls "ill-discoverers, who think there is no land, when they can see nothing but sea." Such, indeed, are these natural disparities, as in many cases to preclude any mutual understanding or communion of the reasoning power.[1]

[1] A regard to this point seems necessary to explain certain records of Animal Magnetism, Clairvoyance, Lucidity, &c., honestly believed by many; which would, if verified by sounder proof, alter all our views of physical phenomena, of the natu-

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In commenting on these subjects we cannot rightly exclude the remark, that a certain amount of speculation, duly guided, enters into the purest form of inductive science. Even could the mind be restrained from excursions beyond the strict bounds of evidence, such restraint would be a wrong done to that genius, which has often through these speculations opened a path to new and more certain knowledge. Instances of success, thus obtained, might be drawn from the history of the most eminent philosophers, and from the records of every part of physical research. It is not hypothesis, but conclusions founded on false or faulty evidence, which are the bane of all philosophy.

I must further add here, that in questions of medical evidence there may be an excess of scepticism as well as of credulity. Sometimes this occurs in effect of a temperament of mind (not uncommon among thinking men) naturally disposed to see all things under doubt and distrust. There are other cases, where the same feeling, not originally present, grows upon the mind of physicians who have been too deeply immersed in the details of practice. The hurried passage from one patient to another precludes that close observation which alone can justify, except under especial circumstances, the use of new remedies or active methods of treatment. From conscience, as well as convenience,

of man, and of the Providence ruling in the world. The default of just and unexceptionable evidence which we find here, is still more singularly shown in some recent works of higher scientific pretensions, in which the views and alleged discoveries of Baron Reichenbach are promulgated; upon faith in experiments, so entirely wanting in all that gives exactness and truth to scientific research, that they would hardly be accepted for results of the greatest antecedent probability. The most obvious sources of error are unseen or unprovided for; even such as vitiate the experiments in their first stage of progress. Yet these researches are conducted and related by men of honor and good faith; but wanting in that perception of evidence which is essential to the attainment of truth.

In another volume ("Chapters on Mental Physiology") I have had occasion to treat more largely on these subjects, in connection with the several psychological and physical phenomena, out of the truth and reality of which these false conclusions have been wrested, in defiance of all just proof and reasoning. Still more recently, other analogous, but more puerile, deceptions (table-turning, spirit-rapping, &c.) have yet further played upon the credulity of the world; tarnishing the name of science by assuming it, and rapidly superseding one another in the fashion of the day. In the relation of these ephemeral follies to the great discoveries which have shed lustre on true science during this period, one may find a striking commentary on the maxim of Goethe—"Where there is much light, the shadow is the strongest" (*Wo viel Licht ist, ist starker Schatten*).

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they come to confine themselves to what is safe, or absolutely necessary; and thus is engendered by degrees a distrust of all that lies beyond this limit.

Though such scepticism be less dangerous than a rash and hasty belief, it is manifestly hurtful in practice, and an unjust estimate of medicine as a science, both as it now exists, and as it is capable of being extended and improved. No one can reasonably doubt that we have means in our hands, admitting of being turned to large account of good or ill. Equally unreasonable would it be to distrust the knowledge gained from a faithful experience as to the manner of using these means—and others which may hereafter become known to us—safely and beneficially for the relief of disease. The actual progress of medicine during the last thirty years, in all that regards the principles of the science, as well as the details of practice, is the best testimony for the present and assurance for the future. As respects the two extremes, noticed above, it is certain that there is a middle course, which men of sound sense will perceive; and to which they alone can steadily and consistently adhere, amidst the many difficulties besetting at once the judgment and the conduct of the physician.[1]

I am the rather led to these remarks on the nature of medical evidence, and the causes moral as well as physical affecting it, looking at the wonderful advances which have lately been made in all other branches of science; not merely by the addition of new facts, but yet more by new methods and principles of research, by instruments of greater delicacy, and by increasing exactness of details in every point of inquiry. The effect of these changes has been (and it is a result the grandeur of which can be understood only by those who have pursued their progress) to bring the physical sciences into much closer relation with each other, pressing them on by converging paths towards that unity of principle which is the object and end of all true philosophy. I cannot here do more than allude to the profound researches through which dynamical forces—the phenomena of chemical, electric, and magnetic actions—and those of light and heat—have been submitted in so many cases to common laws;

[1] Laplace happily expresses the middle course here designated: "Egalement éloigné de la crédulité qui fait tout admettre, et de la prévention qui porte à rejeter tout ce qui s'écarte des idées reçues."

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and the whole associated with the great doctrine of *atomic elements*, acting and combining *in definite proportions* throughout every part of the material world. These generalizations, great though they are, do not narrow the field of future inquiry; but give fresh space, new objects, and higher incitements to those who may pursue such researches hereafter.

The dissimilarity of the proofs, and the greater difficulty of their certain attainment, must ever keep practical medicine in the rear of other physical sciences, notwithstanding its closer association with them through the laws of animal physiology and organic chemistry, so largely explored of late years. But its still wider scope of usefulness requires that this distance should be abridged as far as possible; and no occasion be lost—by improved methods as well as by new facts—by more cautious observation and more exact evidence—of maintaining its place and connections among the other great objects of human knowledge.

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CHAPTER II.

ON HEREDITARY DISEASE.

THE subject of hereditary temperament and tendency to disease is still largely open to inquiry. Future research will unquestionably lead to an increased estimate of the influence of this cause on all the conditions of individual life, and not least on those of disease. Looking to analogy in plants and lower animals, we see in every direction the wide and various transmission of incidental varieties in each species; generally depending on combinations of natural causes; sometimes effected by human care and design. One general fact, as respects man, seems to be that so ably developed by Dr. Prichard; viz. that all original or connate bodily peculiarities tend to become hereditary; while changes in the organic structure of the individual from external causes during life, commonly end with him, and have no certain influence on his progeny.[1]

I know not that this principle could have been in any way anticipated, or otherwise derived than from actual observation. It must be received, however, under certain limitations. Every hereditary peculiarity must have had its beginning in some amount of deviation, however small, from the normal condition of the species; multiplied, it may be, by repetition in successive generations of the causes which first produced it. Examples illustrative of this may be found, as well in the wide circle of structural changes from the effect of disease; as also more notably in those instances where necessities of situation alter certain parts of the organization of animals, and where the continuance of such change is needful to their preservation under these new circumstances. The effects of domestication come under the

[1] Researches into the Physical History of Mankind.

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latter view, where changes originally individual to the animal are propagated to its posterity; reaching gradually, as it would seem, a certain range of deviation for each, beyond which definite causes of original structure interfere to prevent further alteration.[1] Nevertheless, as a general expression of fact, the principle just stated may be deemed highly probable, if not wholly proved; and it is one fruitful of important inferences, both in physiology and pathology. It comes in direct relation to the subject of hereditary diseases: and though the manner of their transmission is still a mystery, hidden in the same obscurity as the more general fact of the reproduction of the species, yet are we able to reason upon the effects, and to class them in certain relation to each other, and to the healthy and natural condition of the human frame.

It would be needless, and beyond my purpose, to cite the numerous examples of hereditary peculiarities of conformation, function, and tendency to disease. Such instances are familiar to us in common life—in history and books—and yet more especially and frequently in the practice of medicine. I have, indeed, been led to put together these remarks on the subject, from the numerous examples I find among my notes and recollections; to which I am desirous to give a more definite form, illustrative of their relations to each other, and to the general laws of animal life. As I have already remarked, we are still only partially acquainted with the number and singular variety of such cases. The greater exactness of modern observation is ever placing before us new and wonderful instances, in which the most minute peculiarities or defects, in structure and function, are transmitted from one generation to another.[2] Scarcely is

[1] If other animals than those now possessed were needed for the uses or luxury of man, there is little doubt that human care alone might greatly extend the number of such varieties; and this in truth has been done, in many instances familiar to our knowledge.

There would seem to be cases, where certain combinations of muscles, or faculties of action, habitually called into exercise for particular effects, may produce bodily results more or less permanent, and capable of being conveyed to offspring. The pace of cantering in the horse, the motions of the tumbling pigeon, and the habits of certain varieties of the dog, may be noted in illustration of this fact.

[2] When Savage speaks, in his bitter vein, of

"The tenth transmitter of a foolish face,"

he scarcely exaggerates what is often seen in families, where some strongly-marked feature or expression long continues dominant, or breaks out again in successive generations.

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there an organ or texture in the body, which does not afford its particular proof of these variations, so transmissible;—and we might almost doubt the permanency of the type of our species, thus largely and unceasingly infringed upon, were we not permitted to see something of those more general laws, by which the Creator appears to have set limits to the change, and made even the deviations subservient to the welfare of the whole.

For in considering this hereditary tendency to disease, whether arising from structural or less obvious cause, it is needful to regard it in connection with, or even as part and effect of, that great general principle, through which varieties of species have been spread over the globe, with obvious marks of wise and beneficent design. That there are certain determinate laws, by which the permanence of species is secured, must be admitted as a general fact—the result of all observations hitherto made on organic forms, both living and extinct. The very circumstance of the frequent repetition of analogous and parallel series of forms, under different physical conditions, and at different geological epochs, though admitting some doubt as to its interpretation, is nevertheless most favorable to this opinion. Still we are not entitled to deny to the followers of Geoffroy St. Hilaire the *possibility* that it may be otherwise; and that time may hereafter disclose to us some evidence that species are not immutable. It is, in truth, one of the great remaining problems of natural science, to determine whether the variations induced upon any *specific form* of animal life, can ever become such in amount and fixedness as to constitute a new species, in the complete definition of the term;—a question not to be settled "within the hour-glass of one man's life," but requiring ages of continuous observation, even for the chance of solution.

Our present inductions, though chiefly founded on negative proofs, are, as I have said, strongly in favor of the permanence of species; and there is nothing yet entitling us to pass this boundary of our knowledge. I have always felt surprise, when reflecting on the question, that so little attention should have been given to the argument derived from perpetuation by sexes. Seeing the precise adaptation of the male and female of every species to their relative parts in the great function of generation, those who vindicate the notion of a transmutation of species

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must suppose this exact relation and adaptation to be continued for each sex, through every progressive step of change—a supposition stretching so far beyond all probability, and involving, indeed, such endless adverse chances, as scarcely to allow its being admitted within the scope of argument. If, however, proof were attained of the mutability of species, this would in no wise affect the great argument of Natural Theology. A law presumed at one time to be absolute and universal, would be found collateral or subordinate to a still higher law;—further removed, it may be, from our comprehension, but involving the same proofs of design as the basis of the whole.

Recurring to our immediate subject, we may remark that the conditions upon which the varieties in species of animal life depend, and by which they are limited, allow wide scope to their existence without injury or disorder of the general system. In the instances where such effects do occur, we can discern some part of the modifying causes, even amidst the profound obscurity which conceals from us their original source. And here again we come upon another of the great questions in physiology, which are still awaiting solution from more complete research. The subject of this chapter, indeed, cannot be rightly pursued, without referring to those recent inquiries into the original types and development of animal life, from the simplest to the most complex forms, which form a new field within the domain of human science. Such researches into what has been termed Embryology and the theory of types, though Hervey and Hunter prepared the way for them, have, until lately, engaged much less attention in England than among the naturalists of France and Germany.[1] The boldness or even rashness, of some of the generalizations attempted, particularly in the doctrine of fundamental

[1] In France, Geoffroy St. Hilaire, Lamarck, Blainville, De Maillet, and Serres;—in Germany, Meckel, Tiedemann, Van Bär, Spix, Valentin, Oken, &c., have been the most conspicuous of those engaged in the inquiry. In the former country, Cuvier sought to stay the extremes into which these doctrines have occasionally been carried. The sound and perspicuous understanding of this admirable man, while ever pursuing the great generalizations to which facts conducted him, had the power, no less important, to resist all inordinate speculation, and to recognize the bounds as well as the capacity of human research. Such eulogium is not incompatible with the admission, that he was in some cases too much limited by his own early conclusions; opposing those attempts at higher generalization, which, though more speculative in kind, were nevertheless the natural offspring of his own methods of inquiry.

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unity of structure and design, must not prevent us from looking to a large increase of sound knowledge from this discussion, based as it is upon a close and minute observation of facts. Its bearing upon the question of hereditary conformation and peculiarities—whether in families, tribes, or races, of men—will be obvious to all who have studied this curious subject, and the controversies and conclusions which have successively grown out of it.

Dismissing the notion of a single primitive germ, or absolute unity of animal type, as a thing not proved, and probably incapable of proof; and putting aside also some fanciful analogies which have defaced the inquiry, we yet must admit that comparative anatomy is ever carrying us back, by successive steps, to simpler and more fundamental types of animal life—to conditions of structure, common to large and seemingly remote classes of created beings. It is still a subject of dispute where this progressive approach to more general forms is stopped by essential differences of type, or whether such divisions do at any point in the series really exist—whether there are separate circles of analogous structure, as defined by the four great classes of Cuvier, or by the more complex systems of some later zoologists—or whether these are but artificial limits, subordinate to a common type pervading all the forms of animal life. These are questions strictly within the scope of research, and the solution of which connects itself with some of the highest objects of natural science.[1] I have never, as already stated, seen cause to think the arguments of Natural Theology at all affected by these inquiries; even when pushed to the speculations of a primitive germ, or original unity of type. Its truth, indeed, is far above the reach of what, after all, are but subordinate researches, even if they could by possibility attain the proofs of what these terms express. The chain is lengthened, and its parts are connected together by new and unexpected links. But still it is a chain of designed organization throughout; and if we simplify

[1] In our own country, this inquiry has of late been pursued with ardor and success by many naturalists. The Master of Trinity in his History of the Inductive Sciences, Professor Sedgwick in his Discourse to the University of Cambridge, Dr. Carpenter in his various physiological works, and Dr. Roget in his Bridgewater Treatise, have discussed these questions, as to general types and unity of design, in a happy spirit of philosophical research. But above all, we are indebted to the genius and labors of Owen, for the highest and most important illustrations which have been given to this branch of modern science.

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the first of these links, it is but to render more wonderful the number and perfection of the varieties which are evolved in definite forms from this elementary structure. If the parts in man have all their analogies or models in lower animals; or if, according to one view proposed, we regard the several organs of the human embryo as passing through all the gradations of lower types, before reaching their perfect development in man; still the great argument remains the same. The progressive elaboration throughout all its stages is definite and designed;—the points are fixed at which each deviation has its beginning from the more common antecedent forms;—the limit of change, as defined by species, is fixed in each particular case. If there be an argument for the unity of creation, more complete and comprehensive than another, it is that which is furnished by the recent progress of comparative anatomy; enabling the observer, by the uniformity of general laws, to predicate from a single minute part of structure all the more important generic characters of the animal to which it belongs.

If now, recurring to the subject before us, we make the progress of development the basis of our views, and follow this from more general forms to those which are successively subordinate—even as far as to families and individuals of the same species—we thereby connect the subject of hereditary aberrations with this more general law; finding reason from analogy to expect that such aberrations should occur, and that they should be carried into extreme variety and minuteness. The different parts of structure and function become more special, or individual, at each step of descent; never losing, however, their relation to the original common type; and in the instances at least of individuals of the same species, tending ever to recur to it, when the causes of change are modified or withdrawn.

This manner of pursuing the inquiry, through the unity or analogy of structure in corresponding organs, is probably in closest conformity to nature, and the most likely to lead to correct conclusions from the facts observed. Among other results already obtained from it, is a more just view as to the nature of what have been termed *monsters;* which, though still regarded as deviations, more or less, from the natural type, are so, for the most part, in the sense of an interference or arrest at particular periods of development; displacing organs only partially evolved,

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or suspending the progress of structure in parts yet imperfect. The advance here, from the old and vague notion of a *lusus naturæ*, is obvious; and the details into which the doctrine has been carried form a remarkable feature in physiological science. Though exceptions exist to the law propounded by some naturalists, that every particular monstrosity in man has its analogy in the natural state of the same parts in some inferior animal, the fact is nevertheless so general as to justify its being regarded as a principle in the development of animal life.[1]

It can scarcely be needful to say, that these views are simply illustrative of relation in a series of effects. While augmenting our knowledge in this way, and leading us up to the threshold of the mystery, they give us no power to pass beyond it. The insuperable nature of the limit is even rendered more obvious, when thus approached, than as it is seen from more distant points of view. While we find cause for wonder at the transmission of resemblances from parent to offspring, we must admit the wonder to be equal that there should ever be deviation from this likeness, and that such deviation should be so little governed by any apparent rule or law. The one case is in reality as great a miracle to our understanding as the other.

The hereditary tendency to disease, regarding the subject in its most general light, shows itself either in the anormal conformation of particular organs or textures;—or in the presence and transmission from parent to offspring of certain morbid products, either altogether new, or vitiated in kind, or faulty by excess.[2] Pursuing the subject more closely, it may be inquired, whether these morbid products may not be all referrible, as effects, to variations in some part of organic structure, producing or evolving them; so that the solids of the body alone, by their conformation and texture, carry on such peculiarities through successive generations? or, whether the animal fluids, also, and par-

[1] The "Histoire des Anomalies de l'Organization et des Monstruosités," by M. Isidore St. Hilaire (1832–36), contains all that is known on this subject;—expounded according to Serres's doctrine of *centripetal development*, in opposition to that which has been termed *centrifugal*, from opposite views as to the progressive formation of organs.

[2] I do not use the term *monstrosity* here, as this in common language expresses only extreme cases of mal-conformation; depending, it may be, on the same causes, but for obvious reasons more generally prevented from becoming hereditary.

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ticularly the blood, may not be concerned in the transmission thus taking place?

This is a question involving much curious but doubtful speculation, nor does the actual state of our knowledge afford any certain answer to it. We cannot go further than to say, that the evidence as to the solid structures of the body, and the changes they severally undergo, is more distinct and complete; but we are not justified in denying that the blood also may take on morbid conditions, directly transmissible to offspring. The question here, in truth, involves the origin of the blood itself, in relation to these solid parts; and merging in this, its import to the subject before us is less than might at first be supposed. So close is the mutual connection between them and the blood, in growth, function, and change, that it is scarcely possible, even were it practically needful, to separate the two in this inquiry. The faulty organization of certain parts of the former may probably produce morbid changes in the latter: or the fluids of the body, already vitiated from other cause, may as probably alter and deprave the texture and function of the solid parts.

If there were reason for going further into this curious question, it might fairly be argued, that, however difficult to conceive a fluid like the blood, ever in motion and change, being capable of hereditary taint, yet is not this really more difficult to understand than a character or peculiarity conveyed by descent to any part of the solids of the body. The blood has vitality in every sense in which we can assign such property to the solid parts. Under some views, it is the portion of the animal frame especially so endowed. Its first appearance in the *area vasculosa* of the germinal membrane of the embryo, is prior to the existence of those very organs which, after birth, chiefly minister fresh materials to it. And though undergoing constant change, it has this condition in common with the animal solids; all which (including those most frequently the subjects of hereditary affections) are unceasingly altered by growth, by waste, or by fresh assimilation of substance. The morbid changes, moreover, which the blood undergoes, are often the effect of inconceivably minute portions of foreign matter brought into the circulation; as denoted in the effect of various poisons, in the exanthematous disorders, and other instances familiar in the history of disease. I might further allude here to the remarkable facts lately dis-

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closed by the microscope as to the forms and magnitudes of the Blood disks, or Corpuscles, as well in man as in other animals;—definite for each species, but with singular diversities in different parts of the animal kingdom. These facts are already numerous and well determined; but at the same time, to our present knowledge, so completely insulated, and offering so many apparent anomalies, that it would be idle and unsafe to found any speculation upon them. It is a problem open to the future; and which, if ever solved, will probably be found to disclose at the same time many other phenomena of animal life, now most obscure to our comprehension.

This part of the subject, then, can be approached only through general views, and subject to the obscurity which overhangs the whole. Yet even these general views, as far as they are reasonably supported by analogy or otherwise, have their value to medical science. We are called upon to recognize certain constitutional diseases, continued from one generation to another, the material of which (if such phrase is permissible for what cannot be presented in separate form) can only be conceived as circulating in the blood, and conveyed thereby to the parts or organs which it affects with the characters of visible disease. These constitutional disorders, and their results, will be found to comprise under a few really specific differences, very many forms of disease, scattered through our nosologies under different names and distinctions. Their relations to certain common morbid principles (an inquiry of great importance in every way) are often expounded or confirmed by looking to the fact of hereditary transmission; and to the agency of the blood, as containing in itself the elements of disease, or as conveying them to different parts of the body.

Contemplating, then, generally, this great principle of the transmission of bodily peculiarities from the parent to the offspring, it will be seen that we cannot yet rightly distinguish what belongs to the blood—what to the solid textures of the body—what to the connections and mutual agency of both. All reasoning here merges indeed, and is finally lost, in the great mystery of generation itself. Nevertheless, we may find in these questions a convenient basis for the general arrangement of facts, where the latter are so numerous and varied as to need some method for their fit observation. Our imperfect knowledge

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leaves much that cannot be submitted even to the broadest lines of distinction; but in this, as in other sciences yet unformed, facts, truly determined, will best lead to correct classification and future laws.

Looking first to examples of the anormal conformation of particular organs, transmitted by descent, and limiting the consideration to man, we find the variety almost beyond estimate. Here, indeed, a view occurs in the outset of great interest to the inquiry, but which I do not think to have been hitherto sufficiently regarded.

If peculiarities of external form and feature, whencesoever originally derived, tend so speedily to become hereditary—affecting, as we see on every side, not families alone, but, by intermixture and descent, whole races of mankind—we can have no doubt that deviations of internal structure (whether they be of deficiency or excess, or of any other nature) are similarly transmitted; and with them propensities to, or conditions of, morbid action in the parts thus organized. Though the direct proof is not equal for the two cases, and though the effects resulting are of such different importance, yet is it certain that the peculiarities, so carried on from one generation to another, have reference for both to one common law. Those deviations from the primitive or common type of the species, which occur chiefly in the bony structure, integuments, or muscular fabric, producing varieties in the outward form and feature, in the texture or color of the skin, hair, &c., may exist to great extent, without affecting in any important way the health or natural functions of the individual. On the other hand, much smaller deviations from this type, in the internal organs of circulation, respiration, digestion, absorption, or secretion—or in the brain and nervous system—may produce morbid actions, painful in progress and fatal in result; each class of deviations alike transmissible to progeny, under the same general law. A faulty texture of skin, a hare-lip, or strabismus—all frequent hereditary defects—may be of little import to health; while a morbid structure of internal membranes, a slight organic deviation in the heart, or a tendency to deposit in the crystalline lens, may inflict danger or distress upon every part of life.

This view, if authentic, is obviously one of singular importance in the history of disease, and capable of very wide appli-

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cation. It throws light upon the connection of various morbid states, by giving the relation to a common physical principle in their cause; and this principle one which is associated with others of the more general laws of life. Here, for example, we must look for explanation of the difference in the average duration of life in different families—a fact well attested in itself, notwithstanding the many exceptions which the laws or social usages of mankind are ever inducing upon it. There is no just reason to doubt that hereditary peculiarities of structure are as frequent and varied—perhaps as extensive—in the internal organs, as in the external parts of the body. Analogy would suggest this as a probable truth. It is confirmed in great measure by observation; and more extensively as the examination is rendered more minute.

On the same grounds it may be presumed that there is a general relation between the resemblance of external features and that of internal parts of structure. The child most like its parent in traits of countenance and figure, has probably closest kindred with him in other and more minute points of conformation. The evidence here is chiefly that of similarity of morbid affections in such cases;—a fact almost indisputably ascertained, and which, if the views contained in this chapter be correct, affords the best proof we can reasonably seek for. In the instance of gout it has often been observed, that the children most resembling the gouty parent have greatest liability to the disorder in its common form. Admitting this, it can only be explained by supposing a corresponding likeness in those parts of structure which are chiefly concerned as causes or seats of the disease.

It must be confessed, indeed, that this inquiry is very incomplete in many of its parts; and, on first view, it might appear that such internal deviations were much less extensive than those of outward conformation. But we can scarcely name any organ of importance which does not afford evidence of diseased actions, derived from structure and transmissible by descent. And looking to the textures more widely diffused through the body—as the different vascular systems, the nerves, &c.—we have every reason to suppose (though the proof is less direct and of more difficult attainment) that they are subject to hereditary variations of structure, not merely in detached parts of each system, but also in the structure of those minute branches and terminations

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where the most important functions of the body, both animal and vital, must be presumed to take place.

On a subject of this nature, however, it is not sufficient to refer merely to the vague division of external and internal parts. The distinction between the animal and vital organs is at the foundation of every such inquiry, and each particular question must be brought into connection with it. The fact may be considered as ascertained, that the vital organs are subject to more frequent and extensive deviations from the natural type than those of animal life; the principle of symmetry (essential it may perhaps be deemed) in the latter, requiring, for the integrity of the functions, that all such deviations should be limited in extent.[1] A presumption might hence arise, that the tendency to transmission by descent would follow the same law, and the parts belonging to organic life offer more frequent cases of hereditary mal-conformation than the animal organs. I know not that this question of relative frequency has ever been explicitly answered. There is, however, enough of proof to make it probable that no disproportion exists of the kind just indicated; and that anormal structure in the parts of animal life is quite as liable to be transmitted as in the organic. Taking the obvious instances indeed, these may appear more numerous; but as the difficulty of observation is much greater in the latter case, it is enough to rest in the fact that both the great divisions of life are liable to this general law, and probably not in very different degree.

Though refraining, for reasons already given, from any long enumeration of the special instances of hereditary malconformation and tendency to disease, yet some must be noticed, either from their singularity, or in illustration of what may hereafter become more definite laws. One point deserving attention is the wonderful extent to which such congenital deviations are carried, not only as respects the variety of the parts affected, but also their individuality and minuteness. Some of the earlier examples I am about to cite will well illustrate this; while others, less special, will show how largely the body, in its morbid as well as healthy conditions, is submitted to the general law. These examples are chiefly taken from my own notes; without looking

[1] It is a remarkable attestation of this fact, that where anormal varieties of the muscles occur (a much rarer event than in the vascular system) there seems a strong tendency in these varieties to become symmetrical for the two sides.

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to the many, more or less authenticated, which might be drawn from medical works, or other sources of information.

An instance is known to me of hydrocele occurring in three out of four successive generations, in one family;—the omission adding to the singularity of the fact, from its depending on a female being a third in the series, in whose son the complaint reappeared.

I am acquainted with a family, in which there are three examples, the father and two children, of inability to distinguish red as a color. Another example, resembling the last, is known to me, where three brothers, and two or three children of their families, have the inability to distinguish between blue and pink. Instances of hereditary defects of this kind are far from infrequent.[1] I have known squinting to occur in every one of five children, where both parents had this peculiarity. An example has recently occurred to me of that remarkable affection, the *suffusio dimidians*, existing in a father and his daughter; and brought on in each by circumstances singularly alike.

The frequency of blindness as an hereditary affection is well known, whether occurring from cataract, or other disease of the parts concerned in vision. The most remarkable of the many examples known to me is that of a family, where four out of five children, otherwise healthy, became totally blind from amaurosis about the age of twelve; the vision having been gradually impaired up to this time. What adds much to the singularity of this case, is the existence of a family monument, long prior in date, where a female ancestor is represented with several children around her; the inscription recording that all the number were blind.

The repetition of cases of deaf and dumb children in the same family is familiar to those who are concerned in institutions for the relief of this congenital defect; and will be afterwards noticed in reference to another singular modification of this fact.

In one family I have known four or five cases of the peculiar *tremor tendinum* of the hands and arms, which is sometimes called the shaking palsy; and which here occurred in young persons of sixteen or eighteen, as well as in those more advanced in life. In this case, too, as well as in another of the same affection, I

[1] An interesting case of this congenital defect, where it was also hereditary, is given in the Med. Chirurg. Transactions, vol. ix, p. 363.

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had proof of the peculiarity having gone through at least three generations.

Defects or peculiarities in the form and setting of the teeth, as well as in the hair and nails, are often hereditary: and left-handedness, from whatever cause it proceeds, I have more than once observed to take the character of a family peculiarity. In a family where the father had a singular elongation of the upper eyelid, seven or eight children were born with the same deformity; two or three other children having it not. I have known another family, in which five daughters resembled the mother in having large growth of hair on the lip and chin. In like manner I have seen enlarged tonsils occurring in almost every individual of a numerous family, without other cause by which to explain it. A case is recently known to me where the patella was wanting both in father and son.

I find many examples among my notes of what must be deemed hereditary tendency to Heart disease; and such, in truth, are familiar to all observers. In one of these, four brothers died, between sixty and sixty-five, of organic diseases of the heart, with prior cases of the same kind in their family. In another instance, which I note here, as having most recently occurred to me, I find three cases, including one of the Morbus Cœruleus, in three successive generations. It is unnecessary to add others.

Of hereditary obesity I have seen some very curious examples; the most recent that of a family, in which, out of two generations, four individuals died of the results of excessive accumulation of fat, producing diseased action of the internal organs, and dropsical effusion,—two other cases of the same tendency existing in the family, under distressing though less urgent form.

Several cutaneous disorders evidently tend to become hereditary; whether from peculiarity in the texture of the skin, or depending on the general temperament and state of the circulating fluids. I have lately seen three cases of Psoriasis in the children of a family where there is strong predisposition to gout; a conjunction I have observed in many other instances. Certain impetiginous eruptions belong also to a family constitution, such as is often termed scorbutic, and are obviously transmitted from parents to children. Ichthyosis is occasionally seen as an hereditary disorder.

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I may notice here another singular disease, the Pellagra of Lombardy, in which, together with the peculiarity of a local limitation, there occurs a very singular succession of symptoms; beginning with a cutaneous affection of leprous character, passing through various stages of cachectic disorder, and ending generally, after the lapse of a few years, in fatuity or death. I have had much opportunity of observing this curious disease in all its forms.[1] There can be no doubt of its hereditary nature, though there is difficulty in tracing it back in Lombardy for much more than a century, and equal difficulty in assigning the causes which give it existence in this district alone. The peculiar form of Leprosy, prevailing in certain parts of Norway, comes under the same description of hereditary disease, and is further analogous to the Pellagra in the fatuity it often produces. In the hospital at Bergen I have seen this singular disorder in all its forms and stages.

Diabetes, from my observation (and Dr. Prout states the same fact) has sometimes an hereditary character. Enuresis in children, from whatever source arising, occurs sometimes in so many individuals of the same family, as to make it almost certain that it has a common congenital origin. What is not less remarkable, as an instance of similar specialty, Emphysema of the lungs has been ascertained to depend in a great number of cases on hereditary influence, independently of any disposition to tubercular pulmonary disease.[2]

Another instance, which may be termed special, though belonging to a part of structure diffused over the whole body, is the Hæmorrhagic diathesis. Though I do not find in my notes any well-marked examples of its hereditary nature, except where confined to the lungs and connected with phthisical constitution,

[1] A paper I wrote on the Pellagra was published in vol. viii, of the Medico-Chirurgical Transactions. There are many points of great interest in the history and pathology of this disorder. Its first appearance seems to have been in the Alto-Milanese; but in the early part of this century, it had spread increasingly over the greater part of Lombardy, and to the shores of the Adriatic Sea. I have seen the disease as far eastward as Friuli, at the foot of the Carinthian Alps. There are some districts in the Alto-Milanese where the pellagrosi form one-fifth or sixth of the whole population. And at the time I visited the Lunatic Hospital at Milan, out of nearly 500 patients of both sexes, about one-third had been brought thither in effect of this disorder.

[2] This fact we owe to the late Dr Jackson, of Boston in America; a physician whose life was too early terminated.

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yet some are recorded so explicit as scarcely to leave the fact in doubt; and remarkable, further, from the seeming limitation of these instances to the male sex. Nor is there greater difficulty of explanation here, than where a more limited portion of structure is concerned. The points of question are the same in each case; and the solution, if ever obtained, must be common to both.

Other diseases might be mentioned, offering questions of the same kind as those stated above. Asthma, for example, sometimes shows an hereditary character. I have known the complaint to occur in three successive generations; and often so numerously in the same family as to make it certain that a common cause was concerned. This cause is presumably one of structure:—yet it would be difficult to affirm it to be so, or to state in what part the peculiarity is likely to exist. It may be that some anormal state of the nervous system is chiefly concerned; there being nothing to disprove the possibility that such state of certain parts of this system may be transmitted by descent, so as to become a source of the disordered actions of asthma. Or the cause may perhaps be sought for in connection with the gouty diathesis, to which I cannot doubt from experience that some forms of asthma are closely related.

Every physician will recognize the general tendency to hereditary character in disorders of the Brain and nervous system. This is a very remarkable part of the subject; involving, as it does, every variety and degree of morbid affection, from simple headache to the worst forms of epilepsy, apoplexy, and palsy. I shall hereafter notice, in relation to another part of the subject, some singular examples in its illustration. The topic is, further, one of deep interest, as including the various conditions of hereditary insanity—instanced not merely in particular families, but even in districts and communities, where from local circumstances there has been little intermixture with the rest of the world. From these facts, well attested to us, we gather the important conclusion, that some deviation in physical structure, whether obvious or not, is the cause of the aberrations it presents. In no other way can we conceive the transmission of the tendency from one generation to another. It may be that a part of the fabric of the brain is concerned, far too minute for the most subtle research to follow: and this indeed might be presumed,

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looking at the nature of the functions affected. But still, whenever the transmission occurs, we are bound by all analogy to infer the presence of a morbid material cause, upon which the phenomena primarily depend.

There is much that is curious in the tendency to headaches thus transmitted by descent, and often going through whole families with similar character. The cause here presumably varies in different instances. Sometimes, and especially perhaps where they are periodical, the affection may belong to the gouty habit, and to the matter of gout in the circulation. In other cases, anormal structure of the vessels of the head may be concerned; in others, again, some peculiarity in the nervous substance itself.

In hereditary affections of the nerves, as in those of other parts, it is extraordinary in what minute peculiarities the tendency often shows itself. It is difficult indeed in some of these cases to distinguish what is due to imitation alone; but in other instances, where this is excluded by circumstances, we find nevertheless nervous habits and disorders of the parents reappearing in the offspring to a singular extent. These entailed disorders are certainly more numerous than is generally supposed; and probably the source of many morbid states, apparently remote in kind. As respects their origin they may all be referred to the general principles we have already laid down.

There are some examples of anormal structure or disease, which though frequently occurring in detached instances, yet are so especially numerous in certain localities, as to afford suspicion, in the absence of other sufficient causes, that hereditary tendency is much concerned. Such is the Goitre of particular districts, no consistent explanation of which has yet been given, founded on local circumstances of climate or mode of life. The Plica polonica, prevailing almost exclusively along the source of the Vistula, is another instance to the same effect. I might apply the same remark, though with greater doubt, to that curious affection, the Trismus nascentium, prevalent in particular localities, and these widely different in all physical circumstances.[2]

[1] Among particular forms of hereditary mental disorder, the tendency to suicide is recorded on what seems to be sufficient evidence. Curious examples to this effect are given by Pinel, Dr. Burrows, and Dr. Rush, of Philadelphia.

[2] When in Iceland, in 1810, I had the opportunity of collecting some facts as to

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The great frequency of Urinary Calculi, in certain districts where there is no obvious peculiarity of air, food, or water, as a probable cause; and the common tendency to lithic acid deposits in particular families, may admit of like explanation. And this is further sanctioned by the certain connection of the calculous with the gouty diathesis.

Seeking then for the most general expression of the facts just indicated, we may affirm that no organ or texture of the body is exempt from the chance of being the subject of hereditary disease. Or, in other words, every part is susceptible of deviations from the normal type or natural structure, capable of being conveyed to offspring; and of producing morbid actions, which are thus, under the name of disease, frequently propagated through successive generations. Of the instances given, to which many others might have been added, it will be remarked that all are perfectly congruous with the common transmission from parent to offspring of external features of the body, in the peculiarities of which no diseased action is involved. The wonder and the difficulty are alike for the two cases.

I have alluded to the distinction which may be made in treating this subject, between hereditary malconformations of particular organs or structures, and those diseases, not less certainly transmissible by descent, where the blood must be regarded as bearing part in the phenomena. Most of the instances hitherto cited belong to the former class; but others can hardly be submitted to either, under the vague knowledge we yet possess on this obscure subject. All the questions, in truth, are of singular difficulty with regard to those diseases, where the hereditary evil is not of one organ or texture, but seeming to pervade every part of the body;—showing itself at intervals, and especially at certain periods of life; and very frequently shifting its action sud-

the singular frequency of this disease in the Vestmann Isles, on the southern coast of this Island. On these desolate rocks, the population of which does not exceed 160 souls, I found that, in a period of 25 years, 186 infants perished of this disorder, under the age of 21 days; of whom 161 died between the fourth and tenth days after birth; 75 on the eighth day. Though the condition of life of these poor people is singularly destitute, fish and the eggs of sea-fowl being their sole aliment, yet is it not so different from that of the Icelanders of the mainland as to explain the frequency of this fatal disorder amongst them; and it would seem as if some constitutional [illegible] hereditary causes were concerned.

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denly from one part to another. Gout and Scrofula in their various forms, and perhaps also the Cancerous diathesis, will at once occur as the most familiar examples. The very difficulty of the research here affords presumption of the extent to which it may hereafter be applied, in extending and correcting our pathological views. In gout, for instance, admitting, what can scarcely be denied, a morbid ingredient in the blood as the cause of the disease, it may be asked whether hereditary gout depends on the transmission of some part of structure, favoring the formation of this matter? or on some faulty texture of the kidneys or other excreting organs, whose office it may be to remove it from the body? Or further, if it be that gouty matter is merely an excess of some ingredient natural to the blood, whether the determination to the joints, forming the fit of active gout, is the effect of hereditary conformation of these parts; the absence of which peculiarity in others allows this excess to show itself in very different appearances of disease?

I have noticed these questions more in detail, when treating elsewhere of this disorder. They apply equally perhaps, though this relation has been less followed, to those forms of acute Rheumatism (a name unfortunately vague in its use), where there is every proof, from the nature of the symptomatic fever—from the peculiarity of the secretions and deposits—and from the metastasis to internal parts—that the disorder is one of the whole habit, and probably depending on some peculiar state of the blood. There is sufficient evidence to show that this disease is often hereditary, and involving, therefore, the same questions as gout; though perhaps more dependent on occasional exciting causes from without, and less marked as a general temperament of body.[1]

Many similar inquiries will apply to the Scrofulous or tubercular constitution; admitting it, indeed, as a single form of cachexia. It may be asked whether this congenital temperament consists in a certain texture of solid parts, vascular or otherwise, giving liability to peculiar deposits at certain periods of life? or in a specific morbid state of the circulating fluids, producing such deposits in the parts of the body most prone to receive

[1] The most ample proof is that furnished by the large experience of Chomel, who rates as high as one half the proportion of rheumatic cases where the parents had suffered under the same disease.

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them? The latter view, as in gout, may be admitted as the more probable one, and for the same reasons. Indeed, however different the diseases in many respects, yet certain general analogies of character subsist between gout and scrofula;—in their hereditary nature—in their respective connection with particular periods of life—in the proofs of a morbid matter peculiar to each—in the various forms under which the morbid cause shows itself in different persons—and in the frequency and facility of translation from one part to another. These analogies lead us to no common cause; and it may be affirmed that none such exists. But they show causes operating by certain common laws; and, in a case where there is still so much to be learnt, all such relations are of value. Hereafter we may be able, by these and similar means, to associate together diseases now widely apart in our nosologies;—gaining thereby a classification more just, and of greater practical usefulness, than any we now possess.

In the Scrofulous temperament, even more than that of gout, we have a remarkable diversity in the forms the disease takes, and the organs it attacks. But it is worthy of note, that there appears a general tendency to the same form in the same branch of a family thus affected; still more in children of the same parents. I may mention, as a striking example, the number of cases of blindness, partial or complete, in some families where this temperament exists. In others, the disposition is as strongly marked to affections of the joints; in others, again, to pulmonary consumption in its more common form. These are instances of the tendency to specialties before noticed, extending to morbid structure or functions, as well as to those of healthy kind; and, in this relation, attesting the principle to which I have alluded in the Preface, as fertile beyond all others in just conclusions as to the nature and treatment of disease.

What has been just said of the scrofulous diathesis applies by parity of all essential characters to the Carcinomatous habit. Though the proofs are more limited by the comparative infrequency of the disorder, it is certain that this also is often an hereditary disposition of body, tending to the production of morbid growths, more especially at certain periods of life. If, as alleged, the lymphatic temperament is that particularly disposed to carcinoma, the question again arises (but without any

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further facility for solving it), whether this connate tendency consists in a state of the solid tissues, or of the fluids circulating through them? In using the term lymphatic habit, we are not, indeed, entitled to carry our meaning beyond some of the more obvious effects of a constitutional cause; the nature of which, whether affecting the solids or fluids, is still unknown to us.

Certain general facts still remain to be stated on the subject of hereditary disease; some of them of deep interest, but of equal difficulty. One of these is, that singular variety in the general law which Duchesne and others have termed Atavism; where a bodily peculiarity, deformity, or disease, existing in a family, is partially or wholly lost in one generation; reappearing in that which follows, or even yet later in succession. A curious instance of this has been already mentioned, where the omission depended on change of sex in the series. There may be many analogous cases, in which the part of structure affected by hereditary disease admits of the malady being disguised, or superseded, by other casualties in the bodily conformation of the individual. Or, if the animal fluids be directly concerned in the transmission, we may conceive these liable to be more readily affected by causes of variation; so that the same morbid influence is directed to one part rather than another, assuming totally different aspects of disease. But these explanations, probable though they may be in part, will scarcely apply to the numerous cases where, the sex and all obvious circumstances being the same, a disease or deviation from common structure is missing in one or more individuals of a family series, recurring in their children. Here we pass at once into the obscurity which hangs over all that belongs to that great function of life, the reproduction of the species. But the same analogy, or rather unity of plan, is still present to us. If one generation escapes the transmission of an hereditary disease, so do we often find some strongly-marked feature of face or figure lost in one of a family, but reappearing in the children. The proof is hereby strengthened of that general relation which pervades all these phenomena, and which makes the simple resemblance of an external feature the exponent of other cases, in which the most severe diseases are conveyed from parent to offspring.

Connected with the foregoing is another curious variety in the transmission of disease, if so it may be termed, which has been

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very little noticed by medical authors. I allude to the case of several children of a family being affected in common with some given malady, of which there has been no certain instance on the side of either parent. An example has lately occurred to me in one family, of three sons and a daughter, every one of whom underwent an attack of hemiplegia before the age of forty-five, though neither father nor mother had been similarly affected. I find another instance in my notes, where three brothers severally suffered hemiplegia, and about the same period of life, without any record of the like event in the family. I have recently seen a fatal case of cerebral disease, with epileptic fits, in a young lady of twenty-four; two sisters of whom had died about the same age with similar symptoms, though neither parent had been subject to such disorder. I am acquainted with a family in which four children have died during infancy from affections of the brain, without any like instances in the family on either side. In another family, without any similar disease in the parents, three or four children had epileptic fits.

I have notes of several similar instances; chiefly, as I think, but not exclusively, disorders of the brain and nervous system. I have known three cases of Diabetes mellitus in brothers, under ten years of age, in the same family; one of them fatal in result. In another instance, four cases of ascertained disease of the heart, all fatal about the same period of life, occurred to my notice in the brothers and sisters of one family; without any suspicion, as far as I could learn, of the parents having been the subjects of this disease. In the instance of the deaf and dumb, already referred to, the examples are frequent and curious of several children being thus affected (five out of a family of eight, four from a family of seven), without similar defect existing in the parents. In one family with which I am acquainted, three children out of five are dumb, and of deficient intellect, but without deafness; there being here also no record of the like defects in the families on either side. At the School for the Deaf and Dumb in Manchester, in 1837, there were forty-eight children taken from seventeen families; the total nnmber of children in these families being 106, and giving therefore an average of nearly three such cases in each family. Out of these instances, there appears but one in which the defect was known

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to exist in either parent; and we may rightly therefore consider this as one of the most striking examples of the fact under consideration. As a curious variation of the same general conditions, a case is known to me, where the children of parents, each deaf and dumb, are themselves free from that defect. In another large family I have known almost every child short-sighted without either parent being so.

Some of these examples may probably be referred to the condition last-mentioned, of the revival of an hereditary disorder; absent, as far as outward appearance goes, in one or more generations. But proof to this effect is wanting in other cases: and the remarkable fact remains of several children of the same parents being affected in common with a given malady, of which there are no certain prior examples in either family. The extent and sudden development of disease in these instances may in itself be taken as the proof of new elements being introduced.

All physiology is at fault as to the solution of this phenomenon. Nor in a matter so obscure, can we venture beyond the inference, that where each parent is capable of conveying certain characters of resemblance, physical and moral, to their offspring, the combination of these generative elements, whatever they be, must have effect, more or less, in modifying the result. This presumption does not go beyond the fair bounds of reason; is justified by various experiments on other animals; and may afford the clue, under future observation, to some of the most remarkable facts in the physiology of man.

A yet further variety in the hereditary transmission of disease is its limitation, more or less complete, to the males or females of the family affected. This fact is illustrated by two or three of the instances already given, and seems well attested in the case of the hæmorrhagic diathesis.[1] In the more familiar example of gout, the same condition may be admitted to a greater extent. The circumstance most remarkable here is the evidence of transmission through the female of a morbid tendency or action, not observable in herself in its more ordinary forms,

[1] Examples to this effect will be found in the Archives Générales, Oct. 1833, and July, 1835; one or two of them made more remarkable by the transmission of the diathesis through females, themselves unaffected in this way; and further by the very frequent concurrence of the hæmorrhagic with the rheumatic constitution.

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during any part of life. This, indeed, may be only a case of the singular phenomenon of atavism, already noticed; but it is more interesting to physiology, as being more special in kind; and including the relation of the sexual peculiarities to the other functions and changes of the system. Considering the subject in its most general light, it appears certain that the material of morbid action in such cases cannot be altogether dormant, though not attested by the usual signs of its presence. We may best, perhaps, remove the difficulty by looking to differences in the seat and manner of action, whether depending on sexual structure, or on other less obvious conditions. It cannot be doubted that such differences exist, modifying the influence of every one material agent upon the body; and thereby giving, it may be presumed, a totally different aspect, as well as reality, to the effects of the same physical cause. I shall have other occasions to remark on the wide application of this principle to the phenomena of disease.

Looking at what may hereafter be learnt on these curious subjects, the most obvious source of knowledge is the observation of corresponding facts in other animals. Here our means of inquiry are greatly increased by adding direct experiment to the mere observation of facts. The results obtained from breeding, especially in the domestic animals, are infinitely valuable in this respect; and replete with curious inferences, all more or less applicable to the case of man; though, from some cause or other, they have been less closely applied to this illustration, than their importance might warrant. The whole subject of specialties of structure and function, whether natural or obtained by artificial means, is full of the same interest; and forms the most direct line along which to carry research into the types and development of animal life. The physiology of plants affords similar illustrations; though more remote, from the wider difference in their organs and mode of existence.[1] Much has been attained on these subjects; but more yet remains behind. Though the principle of life and reproduction is still a mystery

[1] I may cite, as an excellent example of the mode of pursuing this research, the work of Gallesio, little known in this country, "Storia della Riproduzione Vegetale." Pisa, 1816. Gallesio takes the genus Citrus as his subject, and, from his experiments upon these plants, draws conclusions which apply largely to the production of varieties, monsters, and hybrids, in vegetable life.

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at the root of the inquiry, all the effects resulting from this principle may become better known, their connections determined, and the relations explained by which they take the forms of hereditary disease.

In a former part of this Chapter, I have adverted to the strong hereditary tendency manifested in disorders of the brain and nervous system, and to the many remarkable results thereon depending. It would be impossible, in a general view like the present, to pursue this subject through its multiplied bearings, interesting though they are to all the conditions of life and human society. Here, indeed, as so often before, we are called upon to note the close relation of particular morbid phenomena to more general laws; and, in the present case, to those laws which determine the varieties of character in nations and communities of men. Hereditary deviations in excess, such as come under the character of insanity, are for the most part corrected or limited, as they arise, by the usages of social life. The lesser variations from the common type (if we may apply this term of type to mind as well as body), which are not so controlled, may, in the infancy of any community, and in combination with other causes, become the basis of those more permanent traits which we designate as the character of a people or a race of men. Diversities existing, as they actually do, under infinite variety, and in many cases created and perpetuated within human record, must be derived in some part from this source. Or, if we could suppose it otherwise as to origin, we must at least admit that this cause is largely concerned in giving them permanence among races of men.

I need not stop here to denote the deep interest which belongs to this argument, in its relation to the physical and moral history of Man on the globe. It is one of the great questions which modern science has put forth—incidentally and obscurely at first, but now better defined as a specific object of future research; and connecting itself with those various conditions of climate, of government and social usages, of migration and intermixture of races, and of diversities of language, which are all associated as integral parts of this inquiry,—illustrating each other, and ever converging towards common and higher laws.

As another remarkable fact connected with the same subject, I may mention the frequent occurrence of an hereditary tempera-

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ment, in which are blended together the elements of various diseases, both of body and mind, with some of the highest mental qualities and endowments. Instances are frequent, where in the same family we find almost every individual possessing some strongly-marked peculiarity—such peculiarities often very different in aspect, yet all having reference to some single point in constitution from which the several effects diverge. Or the life of one individual, so circumstanced, may itself afford example of the most singular extremes of state, equally to be referred to some common hereditary cause.

Another fact still to be noticed regarding hereditary constitution, in its relation to disease, is, the disposition of individuals of the same family and generation to be similarly affected under any given maladies; even such as have no apparent connection with the peculiarity of family habit. There are indeed various chances of error in observations of this kind; and many exceptions will occur. Nevertheless, examples free from ambiguity must be familiar to every medical man, where disorders assume a certain marked character in the same family, as regards the severity of the symptoms, the organs especially attacked, and the effects they leave behind. Such is the case with hooping-cough and various infantile complaints—with exanthematous and epidemic diseases—and with others less definite in their course of symptoms. Among the exanthemata, scarlet fever may be especially noted as an example. In the instance of twins (the frequent occurrence of which is itself observable as a peculiarity in some families), these coincidences show themselves in still more remarkable degree. And it is likely, in general, that they should be more distinct in early life, when extraneous causes have yet done little to alter congenital likeness in structure and habit.[1]

I find in my notes some curious instances, where two children of a family, with strongest likeness to each other in feature, figure, and habits, have shown singular resemblance in the symptoms of the disorders affecting them, and in their idiosyncrasy as to particular remedies.

No new difficulties occur in the explanation of these facts.

[1] An instance is known to me, where, in the same family, three persons attacked with Cynanche parotidæa all underwent that singular metastasis of inflammation to the testicle, occasional in this complaint.

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Every morbid cause must have definite effect on certain parts of structure; and, if these are alike in two or more individuals from family descent, the presumption is, that any diseased actions ensuing will exhibit corresponding likeness in kind and degree. What we might here expect on theory, we find to be realized in the facts. And connected with these is the result, well attested by experience, that there is a general tendency towards the same duration of life in those of the same family;—often modified or subverted by individual constitution or by the incidents of life, but never perhaps wholly absent. In some instances this may be referred to the likeness in particular structures; but we must probably look still further and deeper for the causes concerned in this remarkable effect. The research, in fact, brings us again to the great mystery of the transmission of life itself; —under the condition of a fixed average duration to each species, and with subordinate variations belonging to particular series in the same species.

It is another fact in the history of hereditary diseases, meriting attention on various accounts, that many of these have a well-marked tendency to evolve themselves at particular periods of life, differing for each. This is true, not only as to those which are termed constitutional, such as gout and scrofula, where it might more easily perhaps be anticipated; but applies also to some cases where the hereditary disposition seems limited to a particular organ, as the heart, the liver, or the brain.[1] These circumstances find their explanation in the general views already stated. When the disease depends upon anormal conformation of some particular organ, it may be brought into activity, either by the accumulated effect of exciting causes long continued, or by the operation of new causes, coming into operation at certain periods of life. If there be cases in which we may suppose the blood concerned in transmitting hereditary taint, a point already considered, still it is conceivable (especially as we know that this fluid undergoes actual alteration at different ages) that some changes in its quality or distribution may bring the morbid

[1] I find some curious instances to this effect recorded by Boerhaave: "Novi in hac urbe familiam, in quâ omnes certâ ætate schirrum accipiunt, et hoc malum ab ovo parenti liberis est communicatum;—uti et familiam, ubi omnes certâ ætate icterum accipiunt, et sic orto postea hydrope, moriuntur. Novi etiam aliam familiam ubi omnes primò satis faceti, sed certâ ætate in melancholiam incidunt."—*Boerhaav. Prax.* sect. 485.

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principle into activity at one time rather than another. Or, without referring to such direct alteration in the blood itself, the changes successively taking place in the solid tissues of the body, particularly those of secretion, may so operate upon this fluid as to cause the commencement at a certain period of morbid changes, whether of deposit or other kind of action, which had not before occurred. To attempt explanation beyond these very general views would be merely to substitute vague phrases for real knowledge.

I have not hitherto alluded to the question, which has been discussed and differently answered by different authors, from which parent the predisposition to disease is more frequently derived? The difference of opinion here probably results from the actual approach to equality in the occurrence of the respective cases. The question, in fact, merges in the more general one as to the transmission of physical resemblances from parent to offspring; these being, as we have seen, the chief source of similarity in morbid affections. And as such resemblances, both of lineament and structure, proceed from each parent, according to laws of which we are ignorant, but seemingly in proportion nearly alike, the diseases therewith connected must be considered under the same relation, and as being probably derived equally through the two sexes.[1]

Viewing the subject of hereditary disease in all its parts, one question still arises, viz., whether, and to what extent, there may be a continued progress of change in the organization, or other material cause of disease, through successive generations? or, what are the limitations to such changes, and whence derived? The general answer to this question may be drawn from the views already stated; and particular facts in illustration of it are best deduced from the growth of varieties in animals and plants, whether casual or designed. Natural History, in its present state as a science, furnishes exuberant evidence upon most points of the question; and future research in the same direction is sure to enlarge and better define this knowledge. The

[1] Dr. Nasse, of Bonn, treating of the tubercular diathesis, considers that the disposition to disease is more commonly derived from the mother than the father. The general opinion seems to have been the reverse of this; but neither view, as far as I know, is supported by such amount of averages as to warrant its adoption.

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general conclusion presumably is, that the repetition of circumstances producing variation tends in itself to augment or confirm this, whether disease be the effect or not;—but that the original type of the species is ever present in opposition to the changes thereby induced; defining their extent, differently perhaps for different parts of the structure, but still with an eventual and certain limit to all.

It is unnecessary to point out the important relation to practice of all that concerns hereditary tendency to disease. The subject is one which meets us at every step, and to which our attention is perpetually required, as an exponent of symptoms, as affording some of the most certain means of prognosis, and as directing us in many particulars to the right course of treatment. No judicious physician will neglect the resources hence derived, which are in truth essential to sound and successful practice. It is probable they will be largely augmented in future, both by more exact and ample observation of facts, and by the extension of our knowledge of principles in this remarkable branch of physiology.

There is yet wanting to our medical literature a work which may embrace the subject in its whole extent, and with the aids derived from other departments of science. What I have given here is but an outline of this; and the examples are drawn chiefly from my own observations in practice. These of course might be greatly extended in number and variety, as well from the results of individual experience, as from research, even the most superficial, into the writings of medical authors.[1] I must add, however, that an inquiry so instituted, would require much more specification and exactness than I have been able to give in this general view. It is not enough, for instance, to speak of diseases of the heart collectively, at a time when the morbid anatomy of this organ is minutely investigated, and averages of weight and admeasurement obtained for its condition in different individuals, and at different periods of life. It will be readily perceived that the more precise and minute the definition of all parts subject to hereditary affection, the greater is the hope of further insight into this remarkable phenomenon,—the more certain the aids afforded thereby to the treatment as well as theory of disease.

[1] In looking over the works of Morgagni, for instance, I have been struck by the number of cases of hereditary disease to which he incidentally refers; rendered more valuable as examples by his exactness as a narrator.

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CHAPTER III.

METHOD OF INQUIRY AS TO CONTAGION.

After all that has been written upon contagion, in angry controversy or sober reasoning, I cannot but think that the question still admits of greater precision in the *methods of inquiry;* whereby to reconcile the contradictions, and remove in some part the difficulties, which still perplex it in so many particular cases.

I take the subject in its most general form; and the term contagion in its broadest application; as implying the communication of disorder in any way from one person to another, without reference to time, or medium of communication. The term *infection*, being less limited in its derivative meaning than contagion, may be preferred by some; but using the latter word in the wide and popular sense just stated, it cannot well be misunderstood. The particular proofs by which the contagious nature of certain diseases is established, and the enumeration of these diseases, important though the topics are, do not enter into my present object.

I believe it may be assumed, that in every instance of communicated disorder of the same kind (except the ambiguous case of certain nervous complaints, where other principles are concerned) there is a *materies morbi*, a material cause—whatsoever, and however subtle, its nature and manner of transmission—which is directly concerned in the communication. This assumption, though large, is justified by observation as far as it goes;—by strict analogy and probability, where observation cannot be had. It is likely that future research will rather augment than lessen the number of instances where such miasma or material cause of contagion may be presumed to exist. And it is further probable that we shall hereafter acquire more intimate knowledge of the

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nature of these morbid matters; of their relations to each other; and the manner in which they are transmitted so as to propagate disease. The course of modern inquiry directs through many channels to these important results.

Looking singly, however, to the principle just stated, and admitting its truth, we shall find basis in it for all reasoning on the laws and anomalies of contagion; that is to say, for all the conditions which determine and modify the communication of disease from one person to another. A disease is communicated by some morbid matter, thrown off from the first, and capable of producing like symptoms in the second—when conveyed either by inoculation, by simple contact, or indirectly through some medium of transference. Here then three main conditions present themselves, each open to many variations; and, in their combination, capable of producing the numberless varieties and apparent anomalies in the laws of contagion. These conditions are—*First*, the state of the person giving the infection;—*Secondly*, the state of the person receiving it;—and, *Thirdly*, the condition of the medium through which the transference is made. I believe that reflection will show the whole subject to be comprised under these three heads; and that we are bound to refer to them severally in all particular questions or instances which come before us for solution.

The first is very important; inasmuch as it includes all that relates to the varying quantity and intensity of the virus itself. This forms a part of the condition of the patient giving the infection, and the most essential part:—one, however, by no means duly appreciated in the ordinary methods of viewing the subject. Though we have no present method of estimating either the absolute quantity, or the energy of quality, of the material cause, or the relation which these conditions bear to one another, this does in nowise lessen the certainty that variations exist in different cases, and have effect, directly or indirectly, in every example of infection. In the case of inoculated disease, we have evidence as to this point, from the influence of dilution in altering or annulling the effects of the virus employed. The modifications arising from the particular period of the disorder; from idiosyncrasies of constitution in those affected; and especially from the variety of the part or textures through which communication takes place, are all concerned in

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this view; and have severally their influence in determining the power of the virus, and the course of propagation of the disease.

Under the *second head*, viz. the state of the person receiving the infection, we have modifications derived from the previous condition of temperament and general health—from the actual health at the time, and particularly from the presence or absence of other specific disorder, counteracting or modifying the virus received—and from the state of the organ or tissue first or most intimately affected by it. All these circumstances needfully enter into a just and complete view of the subject; but none of them, as I think, are sufficiently regarded in our common methods of dealing with it. The latter point in particular has been almost wholly kept out of view, from the great difficulty of reaching it by any distinct proofs. Yet we cannot for a moment doubt that the condition of the part which habitually, or casually, receives any given infection, must determine the degree of the effects produced, and probably in many other ways modify their character and course.

Under the *third head*, of medium of transmission, still more numerous variations may be presumed to exist. Putting aside the obvious cases of inoculation and contact of surfaces, and looking to the atmosphere as the medium in the great majority of instances, we have here the endless variety arising from changes of weight, temperature, humidity, electrical state, and direction of currents—circumstances ever fluctuating in themselves, constantly changing in their combinations with each other, and capable therefore of modifying infinitely the action of any virus thus conveyed, even without regard to the chemical changes which it may possibly undergo during transmission. And further under this head, we have the case of fomites or virus thrown off from diseased surfaces, imbibed by porous bodies, and again emitted—occasionally, as some assert, in a more concentrated form, from this previous absorption.

Duly considering these several points, they will be found, I think, to show adequate cause for all the strange and perplexing appearances of contagious disease. So far from it being difficult to explain why a given disorder should occasionally appear infectious, at other times not?—why it should spread rapidly and virulently in some localities, and not at all in others?—why

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it should affect some persons, and leave others free?—why the cases should be violent at one period, mild at another?—it is rather perhaps matter of wonder that the circumstances are not still more varied and irregular than we find them to be. When there are such numerous and ever changing elements of difference, the combinations of these may well give scope to every assignable variety of result.

It is clear that very many of the contradictions of opinion and statement, as to the contagious nature of certain diseases, may be solved by a reference to these considerations. In all common reasoning on the subject, and even in what has been written upon it, infection is too much regarded as a simple and uniform act; and the virus transmitted as the same in quantity and intensity. Such views, however, carry error into every part of the discussion. If we can dilute the matter of Small-pox, so that it is no longer capable of giving the disease by inoculation, equally may the effluvia of certain fevers, capable of communicating the disorder in one degree of concentration be so diluted in other cases—either in their original emissions from the sick body—or by distance—or from the state of the atmosphere—or by the intervention of other matters—that they lose their power of reproducing the disease in its complete and specific form. Accordingly we find that in these fevers, as well as in diseases more undoubtedly and actively contagious, the proofs of infection multiply in proportion as the causes exist which are likely to concentrate, or give direction to, infectious matter; as stagnant air, want of cleanliness and fresh clothing, proximity of place, particular currents of air, &c. And what is true as to these disorders, will equally apply to many other doubtful or anomalous cases in the history of disease.

In Erysipelas, for example, though its occasional contagious nature is sufficiently proved, the instances of this are comparatively so rare, that it occurs in the light of an anomaly to common observation. The controversy still existing as to infection in Puerperal fever, may probably best be solved by a close regard to the circumstances stated above. Nor can I doubt (having seen cases which go far to prove it) that a patient laboring under genuine Measles may be present to another, perfectly susceptible of the infection, without the latter receiving the disorder; in default of the quantity or other peculiar state of the virus, need-

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ful for its passage through the intervening air. And this point receives further illustration from those singular cases, where an imperfect and irregular evolution seems to occur of an infectious disorder, the actual nature and presence of which cannot be doubted. These various conditions can in no way be so well explained as by looking to the difference in quantity or concentration of a given virus, and to the manner of its introduction into the system—diversities which must be of constant occurrence, and can never occur without some change of effect.

It is another and frequent mistake, in reasoning upon contagion, to consider that the infectious nature of a disease may be disproved, by showing that it has been spread without any obvious communication through man or human means. The two conditions brought into the question, instead of being opposed, are in fact perfectly compatible with each other. If a virus can be transmitted from the body through a few feet of air, we are not entitled from the partial experiments hitherto made to set any limit to the extent to which, under favorable circumstances, it may be conveyed through the same or other medium. This virus, as already stated, must in every case be regarded as a *material agent;* and as such, susceptible of various influence and modification. Common reason here concurs with our actual experience of the transmission of the virus of certain diseases in various ways, and to remote distances.

Notwithstanding all the labors of the time past, the subject of contagious disease still offers a spacious field for discovery—perhaps more extensive and curious than any other part of medical science. In the remarks just made I have sought partly to remove two or three common errors—principally to put into a precise form those considerations or methods which must, I think, be the groundwork of all inquiry on the subject. I have purposely abstained from any but the simplest illustrations, though others might be copiously drawn from all parts of pathology. The methods of research now suggested will apply to every view of the nature of the virus of particular diseases, whether we suppose them of chemical origin, or derived from animal or vegetable life; and, under the scanty amount of our actual knowledge, this general application is, perhaps, the best test we can have of their value.[1] Neither the laws of contagion,

[1] It will be seen that I have, in this chapter, avoided every question as to the

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nor the many collateral questions which have perplexed and angered medical authors, can be rightly settled without some common principles to which to refer for classification of facts, and guidance amidst the seeming anomalies they present.[1]

nature of particular miasmata; my object being merely to suggest those principles of research which are applicable to all. In other chapters, and especially in the last of the volume ("On Animalcule Life as a cause of Disease"), I have taken up some of the particular questions on this subject; though chiefly interrogative, as befits an inquiry in which so much is still hidden from us.

[1] A valuable paper on the laws of contagion, by my lamented friend, Dr. Henry, is contained in the Fourth Report of the British Association, in which will be found the notice of nearly all that forms our present knowledge on the subject.

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CHAPTER IV.

DISEASES COMMONLY OCCURRING BUT ONCE IN LIFE.

THE title, thus including only a single character of these remarkable diseases, might be extended to other circumstances in their history; which, though not exclusively limited to this connection, are still so closely associated that it is impossible to view them asunder. Such are the perfectly definite course of their symptoms in the ordinary form;—the frequency of an external or eruptive stage in each;—and the well-marked power of conveying infection by the reproduction or diffusion of the virus respectively peculiar to them;—all points of the highest interest in pathology; and which, under the relations just expressed, involve some of the most abstruse questions in the whole range of science. Controversy, as might be expected, has made itself busy with various parts of the subject; but has yet done little to remove the obscurity which hangs over it.

In pursuing the above connections, even in their most general form, we seem ever on the verge of some discovery, giving new inlet to the more mysterious parts of the animal economy, whether in health or disease. Such discovery indeed, if hereafter made, is not unlikely to be derived from methods of inquiry in which these relations are directly involved. In their nature it is scarcely possible they should be unproductive to research, however great the difficulties attending it. The simple enunciation of the fact, that certain diseases of definite course, and communicable by some manner of specific infection, do generally occur but once in the term of human existence, abounds in the most curious suggestions, and carries us at once into the higher and more hidden parts of the philosophy of life.

The familiarity of the facts prevents indeed an immediate conception of the great wonder they involve. What in itself more strange, than that a minute speck of matter (if indeed what is actually the virus of disease be ever the object of sight)—in

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other instances, agents wholly imperceptible by any sense, and admitted into the body through unknown channels,—should be capable of exciting there certain trains of morbid actions, determinate in their character and duration; reproducing or perpetuating, if so it may be termed, that which is the principle of infection and element of disease; and, above all, giving exemption more or less complete to the remainder of life, from the action of this principle, or the recurrence of the symptoms depending upon it! What again is the nature of that definite change in the body which makes it thus far inaccessible to a certain disease, yet apparently does not affect any other property or function of the animal economy? Though processes of diseased action are concerned in all this, yet it is a mystery of the same nature as that of generation and growth, and creating similar difficulties to our comprehension.

The consideration of these topics involves certain general views as to the course and changes of philosophical inquiry, at different periods in the history of science. It is impossible not to be struck with the fact, that modern research, armed with all its powers of experiment and exactness of induction, has carried, and is yet carrying us back, in the direction of certain ancient opinions, which have hitherto been viewed only as vague hypotheses, without other sanction than the hardihood or ingenuity of thought which first suggested them. The inquiries respecting the ultimate particles or atoms of bodies, their forms, relative weights, and manner of combination—though accredited to us now by the most exact evidence which number or geometry can furnish—do yet represent in a more substantial form certain theories which may be termed the poetical dreams of antiquity, and were apparently so regarded by many of those who first propounded them to the world.[1]

[1] There is much that might tempt one to enlarge further on this subject and to fetch from the incomparable poetry of Lucretius, passages (stript of their conclusions) which stand in singular relation to the most recent speculations of the present age. But this belongs rather to the history of the human mind, and of general philosophy, than to the subject before us. All indeed who read, as well as write, on medical topics, will find how difficult it is to attain any opinion or truth that has wholly escaped the genius or labor of those who have gone before. This is especially true on every point of theory and general doctrine. Little that is new can here be looked for, except what is expressly founded on further discoveries in anatomy and physiology; and on fresh relations, thereby disclosed, to what was already known.

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Even on points, however, where no such stable foundation is yet secured, the tendency is strong at the present time to carry speculation forward beyond the bounds of actual knowledge. The very advance of science itself—the application of new methods of analysis—the discovery of new and subtle agents, universal as the more palpable forms of matter on which they act—the assignment of definite proportion, forms, and polarities, to matter in almost all its combinations—the successful research into conditions of the earth and of organized life anterior to those which now exist—and, further, the high perfection and power given to instruments wherewith to work in these deeper mines of knowledge;—all such attainments may be said to have forced philosophy upon bolder conceptions than heretofore, and nearer to what seem the ultimate bounds of human research. Still it is the wider generalization of facts which serves as starting-place to these speculations; and they are speedily lost again, if not supported by extension of the evidence on which they depend. The fortunate, but unproved and fruitless, anticipations of a former age curiously approach them on this still uncertain ground; gaining admission into the domain of science through this connection, and thus completing the record of human thought and labor, so directed, down to the present time.

These remarks apply to medicine not less than to other sciences; and in singular manner as respects the relations of the blood to the production and progress of disease. Here modern research, by showing the very compound nature of this fluid; the frequent changes it undergoes in the proportion of its component parts; the generation of morbid matters within the circulating system; and the extraordinary influence of even the most minute quantity of certain substances introduced from without, has sanctioned to an unexpected extent doctrines which, after centuries of reputation and general adoption, had fallen almost wholly into disrepute. There is no more striking feature in the history of medicine than the rapid recurrence to these opinions;—in very different form and phraseology indeed, and now based upon the most minute analysis which modern chemistry can furnish; but still involving the same principle, and even conducting to many similar practical results.

It is difficult, in truth, to conceive how a doctrine, which, even in the infancy of physical science, forced itself into acceptance

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by its obvious nature, and by the very necessity of the principle it involves, should ever have been so far neglected, or supplanted by other views. When the circulation of the blood was unknown, and its uses in growth, reparation, and secretion—in the production of animal heat and of nervous power—scantily defined, still was there evidence enough to show that its alterations in quantity and quality formed a great part in all the phenomena of disease. What has been done by chemists and physiologists of late years, in the examination of its healthy and morbid states, renders this now, if not the richest department of medical science, at all events that which gives largest augury of future advancement. It would be impossible, and useless from their familiarity, to recite here all the ascertained facts which have connection with the subject of this chapter. In the note below I have referred to a few of the number, which are most necessary to be kept under consideration; and to these I would entreat the attention of the reader, as explaining at once the difficulties of the subject, and the methods of research through which they may best be overcome.[1]

[1] The main difficulty in this investigation, and bearing closely on all its relations to pathology, is that of interpreting the state of the blood within the living body by its appearances and changes when withdrawn from the vessels. It is yet a question whether all the principles asserted to exist in this fluid (or even in the organized animal textures) do exist there in life; or whether they are not produced, or their combinations wholly altered, when vital actions cease, and coagulation and the ordinary chemical affinities supervene. The state of the fibrine, of the globules, and of the iron or other coloring matter in circulating blood, are particular instances of this difficulty; but it extends to other ingredients and conditions also, which the most exact observations have not been able to determine or explain.

The fact, however, is now well assured, that the blood does undergo great changes in the living vessels;—that these changes may commence from within, or be excited by agency from without;—and that they consist, not merely in fluctuations of quantity, or of proportion of the watery to the saline or coagulable parts, but also in alteration of these solid parts themselves; and in the generation of new compounds, either by anormal combinations of the elements of animal matter within the circulation, or by direct absorption of substances not previously there.

In regarding those causes of morbid change which belong especially to the body itself, the points chiefly to be noted are,—the absorption into the blood of animal products already found in the system—and the retention there, by deficient or disordered action of the secreting organs, of materials that ought to be separated from the body. It is ever to be kept in mind, in relation to the functions and diseases of the blood, that it contains some parts to be assimilated; others that have passed through assimilation, and are to be removed by excretion. And though the latter in the healthy state may be so incorporated as to lose all distinction in the

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To the views so re-established regarding the blood, we must mainly look for whatever partial solution may be given to the questions before us. It is impossible to seek from any other source the explanation of morbid changes, so diffused in their nature, so active yet defined in their progress;—involving the uniform nature of the fluid (a point not indeed well ascertained), yet under morbid conditions it is manifestly otherwise; and products are found in the blood representing those which ought to have been eliminated from the body; or sometimes, though more doubtfully, such as may have been absorbed from parts under disease. The curious evidence, lately obtained, regarding the occasional presence of pus in the blood, may be named as an instance to this effect.

These and other analogous facts, now familiar to us, show some of the ways in which the blood may become the subject of disease; and all have reference to, though they do not explain, the phenomena which form the subject of this chapter. This is a topic, moreover, on which, as I have mentioned above, speculation may be allowed to pass somewhat beyond the limits of actual proof. Looking, for instance, at the specific differences of form and size of the globules of blood in different animals, and the immediate fatal effect of mutual transfusion in such cases, it is impossible not to suppose that varieties in the number or state of these globules, whencesoever arising, may produce essential alteration in the blood, and through it in all the functions of the body. And again, with respect to the agents which affect coagulation, looking at the singular way in which electricity is found to operate through intervening tissues, we must admit the likelihood that certain definite effects from this cause may take place even in the living body. All the phenomena of coagulation, indeed, are of primary importance in the pathology of the blood; seeing the numerous causes which modify it;—that some of these, as for instance most of the poisons, produce their effect in quantities incredibly small —while others, the alkalies and certain alkaline salts for example, are of such simple kind as to give the liability to continual variations by their amount, and by the state in which they are present.

Many other questions apply to the curious subject of the influence upon the blood of matters not duly separated from it, or re-absorbed into the circulation. As an instance, recognized by medical experience, may be noticed the effects of the semen, or the elements of it, retained in the habit; or possibly re-absorbed again after being actually separated: and, on the other hand, the results when excreted in too large quantity, or at periods of life unfitted for the activity of this function. Of the existence of such effects on the nervous system, as well as upon the functions of particular organs, there can be no doubt. The manner of their production is wholly unknown to us, but we have every reason to infer that the blood is the medium through which they severally take place.

That there is, independently of what has been theoretically termed *innervation* of the blood, some constant influence of the nervous system upon the physical conditions of this fluid, seems highly probable, though the proofs are too vague to be admitted into any argument on the subject. This may take place indirectly, through changes in the capillary vessels; or, directly, by some manner of action on the blood itself. But at all events it must enter into all our views on this part of pathology, and in this light I have spoken of it in another chapter.

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phenomena of fever, of metastasis, and reproduction of the same virus from which they originated;—and analogous in so many points to those natural functions in which we consider the blood to be the agent. Even were the inference to stop here in evidence, and to be ever unfruitful of further results, I know not how this general conclusion could be avoided, or any other reasonably substituted for it. But the methods of inquiry, just referred to, render it very unlikely that the proofs should remain limited as at present; and though it would seem that there are parts of the subject wholly beyond attainment, yet the very narrowness of our actual knowledge gives reason for not drawing the line closely or arbitrarily around us.

The remarks which follow are merely an outline of some of the questions belonging to this remarkable inquiry; with such comments as seem justified by what we have hitherto learnt in relation to it, or by analogies drawn from other parts of physical science.

It might be made a preliminary question, what diseases are properly included under the title of the Chapter; and medical opinions would not wholly agree on the subject. But this is a point of difference upon which I do not enter; satisfying myself by taking for the argument those examples among the contagious exanthemata respecting which no doubt can exist. The Small-pox, Measles, and Scarlatina (admitting the latter name as describing all forms of the disease) are evidently the instances which best fulfil all the terms of the question; and from which probably an answer will be derived, if ever we succeed in obtaining it. I do not include the chicken-pox and cow-pox in this enumeration, seeing the dispute which yet exists as to their nature and relations. Typhus, and possibly other forms of simple fever, have connection with the subject, and the Plague a still closer kindred; and illustration may be derived from all, though less definitely than from the three diseases named above. The hooping-cough is placed in the class of contagious disorders, occurring but once in life, and comes, therefore, within the terms of the inquiry; yet with so much difference of character, and ambiguity in certain points of its history, that there would be difficulty in examining it with the rest.

The great problem to be solved is that of the change in the living body, effected by a given disease, which prevents the same

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cause of disease from again producing the same morbid actions, except in rare instances, or after a certain lapse of time. All other questions connected with the subject must be considered subordinate to this. For, the fact being once recognized, it is a natural and almost necessary suggestion that the train of actions, whence this singular change results, must be definite throughout in course and character. And on the same grounds the pre sumption occurs, as more probable than any other, that the agent in every such disease must be a specific virus, received into the system from without, and evolving earlier or later the series of definite changes which ensue. No less determinate cause can be deemed capable of effects thus distinct and specific in their nature.

The principle of contagion in these cases does not so directly suggest itself. Yet, when to the general statement of the problem just given, we add the fact, that, in certain of these disorders, there is an obvious and large reproduction of the virus whence they were derived, it becomes a ready inference (however difficult of explanation) that in all of them the train of morbid actions must tend to diffuse, by some manner of infection, that which is the essential element of the disease.

Coming then at once to the main question—and basing it, as far as possible, upon known phenomena, or physical analogies, which can illustrate, however generally, this remarkable influence upon the body—we have to inquire, first, in what parts of the body (if in any especial parts) the changes take place which produce these extraordinary effects? And, secondly, in what the changes themselves may be presumed to consist?

The former question is that in which we seem to make nearest approach to something like solution. I have already stated the strong presumption that the blood is the portion of the body most directly the subject of these phenomena;—into which the material of infection is received, and through which the series of actions forming the disease is evolved and terminated. Were argument wanting to strengthen the probability, it might almost be found in the question, whether any other part of the human frame can be conceived equally capable of fulfilling the conditions of the inquiry? Unless we either avow total ignorance, or seek to veil it under phrases which convey no real meaning, we must admit that here alone can explanation reasonably be sought for,

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however remote the discovery from our present attainment. An altered state may indeed be produced throughout every part of the circulating system, from local irritation of a particular tissue, as is familiarly known to us in various disorders of inflammatory kind. But it is impossible to conceive of any other part than the blood being the agent in multiplying, by a long series of morbid actions, the virus or principle in which these actions commenced; and secreting it afresh from all surfaces of the body, as happens in some of the diseases of which we are now treating. This is one of the instances where it is easier, from the nature of the topic, to apprehend the general inference than to express the reasonings on which it is founded. Even language is forced here to become hypothetical, to meet the obscurity of the inquiry.

On this part of the subject, however, we possess more direct evidence in the actual introduction of the materials of certain of these diseases into the blood, and the production thereby of the specific maladies to which they belong;—a result familiar to us in the cases of inoculation, and in two or three other disorders where an animal virus is obviously brought into the circulation; but proved also experimentally in the communication of disease by the blood of the person infected, even where there is no virus secreted in any palpable form. Such experiments are hitherto indeed more limited and uncertain than might be desired. They afford, however, considerable proof that the measles have been produced by inoculation with the blood of patients under the disease; and that in other animals, glanders, canine madness, and the mange have been produced in similar manner. The argument with which we are concerned manifestly gains great weight by the double or reciprocal evidence thus obtained. And if the proof be sufficient to show in certain cases that the blood is the seat of the combinations, as well as the active cause of the transferences, in which a disease consists, we can have no reasonable doubt in assuming this, where the facts are not yet equally ascertained.

These experiments also, if duly admitted, apply to another doubt that may suggest itself,—viz., whether the series of changes, constituting what is called the disease in these cases, is really completed in the blood, so that the virus there formed is merely separated by the subsequent processes?—or whether there are

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not specific actions of secretion going on upon the surfaces of the body, and connected with the eruptive part of each disease, by which its peculiar virus is reproduced, and becomes an element of fresh infection to others? The difficulties with which these questions are burdened, are the same in effect as those which belong to the general doctrine of secretion. They are enhanced by the peculiarity of this being a matter alien to the system, and morbid in its own nature; yet, as we are obliged to suppose, generated or augmented in quantity, in the progress of the disorder, by some process for which we have neither name nor conception. The chief problem here is the true order of succession in the events, or that which may justly indicate their relations of cause and effect; and a fair examination into what we really know on this subject will show how scanty is the amount, even on points which seem essential to any reasoning upon it. In affirming the likelihood that the series of changes, from the reception of the virus to the complete evolution and decline of the symptoms, must take place principally within the blood, we are expressing all for which we have argument either from observation or analogy. But if this fact becomes eventually well established, even without more intimate knowledge of the manner of operation, it will furnish a better basis for all future views as to these maladies.

The same difficulties, equally inevitable, and even multiplied in amount, affect the other part of the question proposed—viz., in what these changes actually consist?—How, above all, can it happen that a given series of actions, arising from a virus received, should thus preclude, except in rare or partial instances, the recurrence of similar effects from a similar cause applied? The bare statement of this question carries with it, we must admit, a sort of interdiction to the hope of solution. While avowing, however, that science hitherto affords us no definite answer to the inquiry, there are nevertheless certain facts and phenomena which seem to lie nearest the truth, or to furnish at least the most plausible methods of research. And upon these it is worth while shortly to dwell;—the rather as they have themselves great interest in the history of disease.

The topics they comprise may briefly be thus enumerated:—*First*, The leading fact already adverted to, that these diseases in their proper form, have a determinate course, from the first

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reception of the virus to the complete re-establishment of health. *Secondly*, The apparent insusceptibility of some persons to receive these infections at any time of life. *Thirdly*, The susceptibility not existing at one time, but renewed at another;—sometimes without obvious reason, but in certain cases with general relation to periods of life. *Fourthly*, The deviations from the regular type of each disease; occasionally in the length of interval between the infection and the first appearance of symptomatic fever; more frequently in the interrupted and incomplete evolution of the symptoms. *Fifthly*, The occasional recurrence of each disease in the same person;—in effect of this incompleteness of its first course,—of change in that state which gives protection,—or of other cause wholly unknown to us. And, *Sixthly*, The facts, partially determined, regarding the incompatibility of these several diseases, their degree and order of mutual interference, and their liability to be disturbed or prevented by other disorders existing in the body at the time;—points all of great practical importance, and involving some of the most remarkable questions in medical science.

These circumstances, moreover, may be regarded as the only elements we possess for inquiry on the subject. For it must be avowed that no certain analogy can be drawn from other branches of Physics—none even from the general doctrines of life, or the phenomena of other disease—which will serve to explain the peculiar mystery of this case. We know, indeed, that incalculably small quantities of certain substances brought into the blood act as poisons there; producing occasionally trains of morbid actions of very definite kind. But these are wanting in the great peculiarity designated in the title of this chapter; nor do they, as in the instance of the diseases before us, reproduce a matter of the same kind, capable of perpetuating the disorder by communication to others.

The closest approximation to a plausible theory on the subject, is doubtless to be found in Liebig's views regarding fermentation and putrefaction; as chemical processes, in which the introduction of a minute quantity of matter, in a certain state of chemical change, is capable of producing the same or analogous changes, throughout a large quantity of matter, especially if in a fluid state. The effects following the introduction of putrescent animal matters into the blood (as so frequently occurs in

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dissection) may be admitted in illustration of this view, which is undoubtedly one of very ingenious and curious application to the diseases before us. Still, while removing some difficulties, there remains the insuperable fact of the general exemption given by these diseases, once occurring, to the future part of life. Such immunity we can in no way conceive as belonging to parts which, whether fluid or solid, are in state of entire and unceasing change and therefore insusceptible, it might seem, of any permanent state, such as that of which we are now seeking explanation.

Before commenting further on the subjects just stated, a condition may be mentioned, necessary as it would seem, and perhaps obvious, on any view of the subject; viz. that the virus concerned in these phenomena must be of *organic* and probably of *animal* nature—the product originally of animal actions or changes, and propagated and diffused by the same. No other conception than this can well be entertained, however impossible it may be to vindicate it in detail. The same comment will probably apply to every case in which contagion is concerned. The regular period of maturation, the definite course of actions and metastases, and the multiplication of the matter of the disease—though not all equally cogent as reasons—strongly enforce the same view. The latter phenomenon especially, which has closest relation to some of the natural processes of life, seems insusceptible of other explanation. Admitting, then, that some animal poisons may enter into the blood without producing such effects, we cannot fairly contemplate the peculiarities of those in question without seeing that they have their source, as well as actual seat, in the functions of animal life.

Upon this consideration arises the question (not indeed of recent date), whether the matters of infection in exanthematous diseases, thus designated by all their characters as animal products, may not be of other than human origin;—belonging in fact to some form of parasitic beings not cognizable by the senses or other express observation, but capable of acting on the human body as a cause of disease, and evolving through their own vital changes the successive stages of these singular maladies. Unwarrantable though this speculation may seem in its boldness and want of direct proof, yet does it derive some support from the extension which modern discovery has given

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to the principle of life under specific forms, where before only the relations of unorganized matter had been presumed to exist; —from the almost innumerable conditions under which parasitic life has now been detected;—and, further, from the peculiarities just recited as belonging to the diseases in question, and especially the reproduction of the virus peculiar to each. In the last Chapter of this volume, I have treated on this subject more in detail; and will here only add, that were such opinion hereafter sanctioned by better evidence, it would not be incompatible with the views I have suggested as to the relation of these diseases to the state of the blood, and to the changes taking place in this fluid.

Reverting now to the several points already stated as characteristic of these disorders, an important remark occurs, viz., that under admission of the truth of the first, all the others have a natural, or even in part a necessary, dependence upon it. If the morbid actions ensuing upon a virus received are, from any cause, perfectly regular and defined in their ordinary course, it may be inferred as likely that the susceptibility to undergo them will vary in different persons and at different times; that any interruption to, or incompleteness of, the regular course of symptoms, may produce disorders of other kind, or liability to the original disease; and that such diseases, for the most part, must be incompatible mutually, or with other maladies present in the body at the time. These circumstances are sufficiently connected for mutual illustration; though far short of showing in what that remarkable change consists, which, when complete, gives exemption for the remainder of life.

The different degree of susceptibility to these disorders, and their incompatibility mutually, or with other diseases, are points which have been much examined. On the latter subject I will merely remark, that we may, in every case, conceive incompatibility to depend, either on some peculiar state of the blood existing at the time, or on some different virus of disease present in the circulation, and producing morbid actions there, which preclude, more or less, any other from coexisting with them. These general questions, important to every part of the subject, are peculiarly interesting in their relation to the history of small-pox, to the modifying action of the vaccine virus, and to the whole theory of inoculation. As such, indeed, they possess

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a practical importance which belongs to few others in pathology. They have been multiplied in number, and have acquired fresh interest of late years, by the increased spread of small-pox, simple or modified, even among those who have undergone vaccination; giving presumption thereby that the influence of this preventive may be lost by time, or the vaccine virus itself be altered or impaired by unceasing use and transmission. In the first edition of this work I treated of these questions, and of the practical deductions arising out of them, in a separate chapter; but, for reasons stated in the preface to the second edition, I omitted this chapter, and do not enter upon the subject here, further than to show its bearing on the topic now before us.

Its obscurity, in fact, would be in great part removed, were we ever to obtain an entire solution of these difficulties. The strong presumption now obtained from direct experiment, that the vaccine virus is but a modification of that of small-pox, and that the difference is due to the passage of the matter of infection through an animal of another species, may be regarded as a great step in advance.[1] All assured facts as to the duration of the protection by vaccination, the causes which modify this, and the true nature of varioloid diseases, are of equal value and similar application. The relation of chicken-pox to small-pox, whether a product of the same specific virus, modified by unknown causes in its effects, or of a different virus having some common principle of action, is another question, less important in practice, but equally bearing on the history and true doctrine of these disorders. And, above all, a knowledge of the circumstances which practically alter the susceptibility to the infection of small-pox, give occasional liability to its recurrence, and regulate the mutual interference of this and other diseases, would seemingly bring us nearest to the principles in the economy of life which

[1] The experiments of Dr. Sonderland, of Bremen, and those more recently made by Mr. Ceely in this country, appear to have decided the identity of the virus, in conformity with the original opinion of Jenner himself. In the chapter on Vaccination, in the first edition of this volume, I have considered not only the problem as it then stood,—and the further questions as to possible changes in the vaccine virus itself from a long series of transmissions, and as to the duration of the protective influence in the body,—but also the subject of revaccination, and the methods by which original vaccination may be best extended and rendered more secure.

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govern these morbid phenomena, and determine their permanent effects on the body.

The measles and scarlet fever afford similar illustrations, though less extended and less striking, from the absence of that singular connection with another form of virus which has become so essential a part of the history of small-pox. The scarlet fever, however, has peculiar relations of its own, which long obscured the views of the most observant physicians, led to a disjointed nomenclature of the disease, and even still perplexes our opinions on the subject. Limited more than measles or small-pox to one portion of life, it nevertheless is subject to greater irregularities than either of them; appears more frequently in complete forms, and seemingly blends itself more readily with other disorders of the body. I shall have occasion elsewhere to speak of some of these relations; but meanwhile I refer to them chiefly in connection with the curious topic of the frequent imperfect and irregular development of this disease.

In speaking of the methods of inquiry as to contagion, in the last chapter, I have alluded to the influence which variation in the *quantity* or *concentration* of any given virus must have in determining its action on the body. Among the contagious exanthemata, none certainly illustrates this principle so well as Scarlatina. Looking at the disorders among children especially, during the seasons when this disease prevails as an epidemic, we find many which strongly warrant the suspicion of their being derived from the same virus; yet so incomplete and irregular in aspect that it is impossible to define them as really forming part of the malady. A great cause of perplexity here, as in every part of the history of scarlatina, is the peculiar form of cynanche attending it, which occasionally appears as the only distinct evidence of the constitutional disorder. These deviations from the regular type, if we may thus describe them, occur in several, and very different forms; all familiar to medical experience, though not always viewed in the connection to which they belong.

One class of these deviations, and the most difficult perhaps to identify, is that of febrile symptoms, generally of adynamic kind, but very irregular in character and progress;—sometimes a certain degree of angina alone, often none that is obvious;—occasionally partial eruptions, frequently none whatsoever;—a

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general disturbance and distress to the habit beyond what the appearances warrant or explain; all these symptoms occurring during an epidemic, and lingering in the body for a longer or shorter time, but not exceeding the character just described.

A second class of anomalies is that, when succeeding to such irregular symptoms, and without any conformity to the wonted time of maturation, the usual appearances of the disease eventually come on, and pursue the common course, with no deviation sufficient to create doubt as to the identity of the symptoms. It is frequently difficult in these cases to determine the time between the original infection and the full development of the disorder; but the interval is occasionally long. I have known instances where there was reason to suppose it from a month to six weeks.

A third deviation, in some degree a modification of the preceding, is that where the disease, having appeared at the ordinary time, is incomplete in some part of its character, or recedes before it has gone through the usual course; reappearing some time afterwards without any fresh infection, and being occasionally more regular in type in this latter occurrence than in the first.

A further description of anomalies, much more familiar and not less remarkable than the foregoing, are those where there has been an irregular or incomplete evolution of the disease; succeeding to which a number of anormal symptoms show themselves; deviating greatly, it may be, from those which were wanting to make up the natural type of the disorder; yet sufficiently akin to them in time and continuity to afford presumption that they are due to the same morbid matter, not sufficiently extricated from the system. Glandular swellings, abscesses in different parts of the body, eruptions on the skin, vitiated secretions from the kidneys and bowels, are frequent examples of this nature. Occasionally they take a graver form; affecting, and with fatal issue, organs already disposed to diseased action from other causes. These sequelæ of scarlet fever are familiar in practice; and few cases can be said to be altogether exempt from them. But in their more marked degree they become a distinct object of inquiry; and very interesting, in conjunction with the preceding anomalies, as an exposition of that principle which recognizes variations in the action of a virus from quantity, in-

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tensity, manner and period of its application, and from the peculiar circumstances of the body at the time.

The illustrations derived from Measles are less striking than those just cited as belonging to scarlet fever; but they are of the same character, and afford similar inferences. We have the same ambiguous symptoms, occurring during an epidemic, without actually reaching the authentic form of the malady. We have instances where the disorder supervenes upon these irregular symptoms, but long after the usual time of maturation:—and other cases where, after distinctly appearing at the regular period, it recedes too early; and again breaks out after a certain time, without fresh infection, and sometimes more severely than in the first occurrence. Of the latter irregularity I have seen two or three remarkable examples. We have numerous instances again, where the sequelæ of the disease, either from incompleteness of its course, or imperfect separation of the morbid products, are various, protracted, and severe.

Following the subject in such manner of inquiry, we are led to what might be called a still greater anomaly in these disorders, were it not that the real mystery lies in the law, and not in the exceptions to it, viz., their occasional recurrence in the same individual, from fresh infection, at a time more or less distant from the original access of the disease. I have already adverted to this in the instance of small-pox, regarded both alone and in its connection with the practice of vaccination. Scarlatina and Measles furnish examples, much less frequent, but often as undoubted in kind. In my own practice I have seen several instances of secondary measles, in which the proofs were unequivocal of prior occurrence; many more in which there was presumption without certainty.[1] In most of the former cases,

[1] Three of these cases happened in the children of one family. The disorder, brought from a school where it was prevailing, affected all three in succession. Though the catarrhal symptoms were slight, it might not have occurred to name any of the cases "*Rubeola sine catarrho*," but for the second appearance of the disease in all. In one it occurred about six weeks afterwards, and without any proof of fresh infection, in a very severe form. In the other cases there were intervals of five and seven years respectively, and proofs of new sources of infection.

Another example occurs in my notes of two or three cases of secondary measles in the same family; serving to show that some peculiar constitution is occasionally concerned in this anomaly.

In the next chapter I shall notice a singular case in which there was evidence of the measles and scarlatina having severally twice occurred to the same individual; the latter each time in close sequel to the former.

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evidence existed of some incompleteness in the first course of the disorder; and usually in the catarrhal symptoms, according to the common opinion on the subject. There is more difficulty in obtaining distinct proof as to scarlatina, from its greater variety of aspects, and from the singular manner in which the angina sometimes takes place of other symptoms. The examples, however, are indisputable, and not infrequent.[1] If we admit, as must be done, that the protection given by the first occurrence of the disease is often incomplete, it seems probable, from observation, that these affections of the throat are the most frequent form of the secondary disorder. Were cases of this nature admitted, in which there is strong presumption of the same virus acting on a susceptibility lessened but not extinct, the record would become more frequent than at present; and, wherever authenticated, singularly instructive in this part of pathology.

These anomalies indeed (if they can strictly be considered such), and especially that last mentioned, are the channels through which we seem to make nearest approach to the truth. The exceptions, as in so many other cases in science, become the best exponents of the nature of the law. Though failing to give intimate or exact knowledge, they point out the direction in which it lies, and the most expedient methods of pursuing it.

The same remark applies to another class of facts, already named as one of the bases of all reasoning on the subject; viz., the varying liability to these diseases of the same person at different times; and the apparent insusceptibility of certain persons to receive some one of them at any period of life. Medical experience furnishes daily examples of these facts, infinitely varied in form and degree. In the case of the small-pox they are most distinct to observation; testified by an hereditary disposition to receive the malady, and evolve it in the gravest form, or even by fresh infection to undergo it a second time; for so alone can we interpret those singular instances where many children in the same family have been subject to recurrence of the disease. The scarlet fever and measles afford similar instances, though more obscure from the causes already noted.

[1] To some of my readers the instance may be known of a late physician of this metropolis, eminent for his learning, who underwent the scarlet fever three several times; probably from some peculiarity of habit in reference to the disease.

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The admission of an hereditary tendency towards these disorders is a very important point in the question, to which I would entreat the attention of the reader. Extending, as it may do in strict accordance with the analogy of other hereditary affections, to communities as well as families, it solves many seeming anomalies in disease, by connecting them with the more general laws of life. And it helps us in some part to explain the *insusceptibility* to, as well as the *proneness* to be affected by, the particular disorders before us. Though we have no distinct proof of protection being given to the child by the previous state of the parent, yet is it intelligible from analogy that this state may have influence, more or less in degree and duration, upon the temperament of his offspring—and, as a corollary to this, that the condition of habit giving partial immunity against a disease, or rendering it less violent in its symptoms, may extend even to races of men, and be widely diffused by descent.

We are not entitled, upon present knowledge, to proceed far in this speculation. The inference, however, is strengthened, in the first case, by the frequent insusceptibility during childhood; and, in the second, by the numerous and remarkable facts which show a varying liability in different races and communities, and at different periods, to receive these diseases, especially in their severer forms—circumstances which cannot be adequately explained by changes in the virus alone. I need but refer to the evidence we have of their extraordinary virulence, when appearing for the first time in particular localities; and to the proofs (fully sufficient for our present purpose) that the Small-pox in particular had a more fatal character one or two centuries ago than at the present time. Even the severe epidemic form which the disorder assumes in any community, at certain and often distant periods, is not without application to the argument, though depending chiefly, perhaps, on temporary causes. These facts, of which the attestations are very numerous, have not yet been collated or applied, as they deserve to be; seeing their importance to every part of the theory of disease, and the deep interest they possess as portions of the history of the human race.[1]

[1] Striking examples may be found in the narratives we have of the first introduction of the small-pox and measles into some of the South Sea Islands. In the "Narrative of Missionary Enterprise," by the Rev. Mr. Williams, it is stated that

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Among other deductions from the above premises is the following, viz., that the change from complete liability to immunity is not always perfected at once, but admits of great modification under the causes creating it; or, as may more explicitly be stated, from variations in that state of body in which the liability consists. This is manifestly an important condition in regard to the diseases we are considering; and insufficiently recognized in the ordinary manner of viewing them. Not two states alone are involved, but probably several, in passing between the two extremes. It may be that these states are continuous, graduating into one another; or we may regard them, on the contrary, as separate and definite physical conditions, each liable in different manner or degree to the action of a given virus of disease. There is no certain evidence on which to affirm either opinion. But, whatever the reality may be, we are obviously working here among the phenomena which form the foundation of the knowledge we seek for—a conclusion further justified by the concurrence of so many separate views in the same direction of research, and even from the singular uniformity in the difficulties they present.

most of the epidemic diseases which raged in the islands while he was there were brought in by ships; and this even where there was no evidence of like disease among the crews of the ships at the time. We have statements to the same effect from various insulated localities on the globe, sufficiently attested by their number and explicitness, to warrant the opinion that the same germs of disease have very different influence upon different races of men, and that repetition of a given disorder through successive generations may alter the liability to receive it under its most severe forms.

A melancholy illustration occurred to my own notice in the case of the King and Queen of the Sandwich Isles, and their seven native attendants, when they visited England fifteen years ago. I attended them, with Sir Henry Halford, during the measles by which they were attacked soon after their arrival in London. The disease, respecting the source of which no information could be had, showed in all a malignant violence of which the examples are rare in this country. Medical treatment was greatly impeded by the almost total want of means of communication; and in the cases of the King and Queen (both of whom died) was rendered utterly unavailing by the violence of delirium, which overpowered all those placed in watch over them, and led to excesses contributing doubtless to the event.

I may mention, as a particular instance, not generally known, of the virulence of the small-pox when invading a country after a long interval of exemption, that in 1707 nearly 16,000 persons perished from this disease in Iceland, the whole population at that time not exceeding 65,000 souls. The accuracy of their records accredits this statement, however startling the proportion of mortality it relates. The measles, also, have occasionally been very fatal as an epidemic in this island.

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We recur then now (with whatever aid these considerations may afford us) to the question, in what the physical changes consist, which thus give protection—partial or complete, temporary or lasting—against a particular disease, without producing obvious effect on other parts of the animal economy? The reality of such changes must be admitted; for without them exemption or liability would become mere abstractions, and all other facts in the history of the disorders be equally annulled. Have we then any sufficient proofs by which to refer them to a particular part of bodily organization? or analogy by which to explain their nature, duration, or difference in degree?

It must fairly be avowed that no facts or analogies we possess will carry us beyond the views already stated. I have pointed out in succession those parts in the acknowledged history of these diseases which seem to bear most closely on the mystery they involve. But none of them go far enough to disclose it; nor are there other physical phenomena which can throw light upon phenomena, the result indeed of morbid action, but intimately blended with all the most essential functions of life.

If an opinion were hazarded on a part of this question, when the whole is so obscure, it would be one which looks to the blood as the actual seat of these wonderful changes. We have already seen the probability that this is the agent in the maturation of the virus received, and in the series of phenomena which form the obvious character of each disease. Though we cannot, even from such admission, derive any certain argument for the ulterior conclusion just named, yet does it find support, as a presumption at least, from some of the reasons previously employed. It is not unreasonable to suppose that the medium, through which this train of actions is carried on, may be that in which their effects are principally and permanently testified. All the circumstances which show varying susceptibility to a given infection, and the phenomena indicating a partial protection from a disorder imperfectly evolved, may best be thus conceived, however imperfect the inference when pursued into details. We are entitled, indeed, to put the same question as before—in what other portion of the body can the alterations needful to this effect be presumed so likely to take place? We cannot suppose the skin, or other membranes, to have become impermeable to the infecting virus; since, in some at least of these diseases, the direct intro-

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duction of matter into the circulation does not reproduce them after having once occurred. The eruptive part of the disorders, whatever its importance in a pathological point of view, affords no argument as to the seat of the changes in question;—or, rather, in the recognized fact that one or two perfect pustules may denote entire protection against small-pox, gives proof that the external textures are not to be so considered. No other organ or part of the body affords the slightest explanation; nor can we shelter our ignorance under any vague appeal to the nervous influence, or the powers of life, so often employed when no clear interpretation is at hand.

It must be owned that the supposition upon which we have here ventured does not advance us a step beyond; or enable us, even remotely, to surmise the nature of an alteration producing this effect, and none other that is apparent besides. Nevertheless, if there be any portion of the living frame capable of answering to this condition, it is assuredly the blood. The remarkable chemical composition of that fluid,—the definite size and figure of its corpuscles, or blood discs—its peculiar physical and vital properties—its alteration at different periods of life—and its great liability to be affected by transient causes of disturbance,—all warrant the conception of changes in its state, numerous, definite, and even of long duration, though in their nature inaccessible to our present means of research.

In the preceding remarks I have not adverted to that curious point in the history of the diseases under review,—the question, namely, whether there is any certain record of them in the ancient writers on physic; or, if not, what is the earliest authentic date of their appearance amongst us? For obvious reasons the Small-pox has chiefly given foundation to the inquiry. Without entering into details of a controversy so familiar, I must state my own persuasion that we have no adequate proof of this disease having been known to the Greek or Roman physicians. Its specific and strongly marked characters—the proportion of mortality it inflicts—and its frequent diffusion under a virulent epidemic form—would all seem to require a description more precise than any which have been quoted from antiquity. Admitting the account of Rhazes as the earliest having any certain application to the disease, it is undoubtedly difficult to conceive why it should then have appeared in this

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part of the world for the first time. Yet is the fact not really more obscure than many other points in the history of these contagious epidemics. Their original generation among mankind is the great mystery of the case, to which all others are subordinate; and for this, as far as we can see, no complete solution is likely to be afforded us, though we may perhaps reach presumptive evidence stronger than any we yet possess.[1]

Reviewing the whole of this inquiry, the following inferences, whatever their respective truth or probability, may be admitted as giving a more distinct form to the various arguments it involves.[2]

First, That the series of actions or changes, constituting these diseases, have their seat in the blood, and are carried on throughout by the circulation.

Secondly, That, although we have no cognizance of the actual physical state or changes which give protection against their occurrence or recurrence, there is more reason for seeking these in an altered state of the blood than in any other part of the animal organization.

Thirdly, That the conditions which render one disease incompatible with another—or produce the various results of their mutual interference—or limit to man their production and development—all probably depend on the same physical principles as the simpler fact of immunity, and have equally their source and seat in the blood.

Fourthly, That the state of immunity, on whatever bodily conditions it depends, is liable to fluctuation in various ways;

[1] This curious historical problem extends, as is well known, to other diseases than those forming the subject of the present chapter; and notably to Syphilis, the Indian Cholera, &c. I may again refer to the last chapter of this volume for some remarks on the subject, as connected with the animal hypothesis as to the origin of the latter disorder.

[2] In stating these conclusions I still refer chiefly to the three great diseases of this class. The difficulty has already been mentioned of giving the hooping-cough any common place with them, even under admission of its infectious nature, and of the peculiar character which the title of the chapter conveys. The fact of exemption from a second occurrence of the disorder is indeed much more doubtful in this case; and what appears such, may be often merely the effect of different time of life in altering the liability to the morbid cause; or giving it direction to other parts of the body, and thereby changing the whole aspect of the symptoms. The latter view, under a more general application, has been repeatedly noticed, as sanctioned by various facts, and leading to many curious inferences.

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—is often partial and imperfect when first obtained—occasionally diminished or lost by time, even where there is presumption of its having been most complete at first—possibly affected in some degree, and after a certain lapse of time, in all;—the rate of such changes being different in different persons, and modified also by the access of other diseases, or by the contingencies of life.

Fifthly, That a constitutional or hereditary tendency exists throughout all these phenomena;—modifying susceptibility—determining, in conjunction with other causes, the regular or irregular course of the disease—affecting its degree of virulence—and giving liability to its recurrence: and that this habit is not limited to families, but extends even to larger communities.

Sixthly, That the several modifications, above named, depend further on the quality or quantity of the virus received in each case—conditions admitting of being infinitely varied; and giving, in conjunction with the effects of season, locality, and human constitution, the peculiar character which every epidemic more or less assumes.

Other inferences might be specified; but the foregoing embrace all that are more important in the general view I have taken of a subject, thus endless in its details, and in the hypotheses to which it has led. Though speculative in several points (an inevitable result of their nature), yet will they be found applicable in various ways, not merely to the theory of disease, but equally so to some of the most difficult and interesting questions which come under our consideration in practice.

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CHAPTER V.

ON THE CONNECTION AND CLASSIFICATION OF CERTAIN DISEASES.

It will probably be one of the most certain results of future inquiry to associate together, by the connection of causes of common kind, diseases now regarded as wholly distinct in their nature and origin, and arranged as such in our systems of nosology. This remark applies very widely throughout all the genera of disease; but in no instance so remarkably as to some of the disorders of contagious or epidemic kind; scarlatina, measles, hooping-cough, erysipelas, the infantile fever, dysentery, epidemic catarrh, or influenza, &c. Without affirming the *materies morbi* to be the same in any two of these disorders;—presuming it to be different in most of them;—and admitting the diversity of the symptoms;—yet relations there probably are, closer and more peculiar than any which have yet been ascertained, or made the basis of classification. And these form, without doubt, some of the most interesting objects of pathological research;—closely connected, moreover, with the remarkable topic, of which I have treated in the preceding chapter.

On the most general view of the subject, numerous conditions will be found capable of giving different aspect even to the effects of a common cause of disease. Season of the year—peculiar states of the atmosphere as to heat, moisture, weight, or electricity—circumstances of locality—the age of individuals exposed—their different temperaments—the influence of foregoing disease—the incidental direction of the morbid cause to some particular organ or texture of the body[1]—and still more

[1] An illustration of this occurs in the *glanders* and *button farcy* of horses; the virus being proved by inoculation to be nearly, if not wholly, identical; but affecting under the former name the lymphatics of the internal surface, under the latter those of the skin.

There is reason for the same inference as to the virus of hydrophobia; the varia-

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the quantity or intensity of the virus itself;—all these and other circumstances may be conceived as producing such modifications, and thereby concealing the common origin of many disorders. From this complication of conditions, the difficulties of inquiry are great, but nevertheless of a nature to be overcome by future observation.

Referring more especially to the disorders mentioned above, does it not happen that measles, scarlatina, hooping-cough, and infantile fever, are often singularly concurrent in particular districts, and about the same periods of time? And as a variation of this fact equally illustrative, is it not common to find one of these disorders exceedingly prevalent in certain localities; while at the same time, in contiguous places, another of them equally prevails? Many instances to this effect come within my recollection; too strongly marked, as it seems to me, to be attributable to chance alone. Similar examples, though often indistinctly noted, are not uncommon in medical authors. If it be alleged that they prove only the influence of certain seasons and particular localities in evolving the virus of two or more of these diseases, or in creating a state of body favorable to their reception, still here are connections which it is important to denote, and to follow into their several consequences.

Morton, as is well known, considered scarlatina and measles to be varieties of the same disorder. It would be difficult to establish this opinion by any proof; and the actual points of diversity may be said to make it impossible. Such are, especially the negative fact, that one disease has never been known by infection to produce the other; and the certainty that the infectious matter of each, though equally unknown to all observation of the senses, has different properties in relation not only to the human body, but also to other media which are concerned in communicating the disease. Even the uniformity with which each disease manifests itself on different portions of the same texture—as the inflammation of measles on the mucous membrane of the air passages, that of scarlatina on the membrane of

tion in the effect of which on different animals may be presumed to depend, not merely on their diversity of habits, but also on the difference of parts more directly or more severely affected by this remarkable poison. The most exact observations we have on the subject are those by Dr. Wagner, in vol. i, of Hecker's Annals, 1836.

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the pharynx—furnishes proof of different origin; and not less the difference in the character of the attendant fever, and of the ordinary sequelæ of the two disorders. Yet still there are some relations which cannot wholly escape notice; and which in fact lead to the occasional difficulty of discrimination, in the early stages, and in certain forms, of each disease. The case related in the subjoined note involves only a succession of the one malady to the other; yet this is so singular, in having twice occurred under circumstances similar as to interval, as to warrant the suspicion, that there was some peculiar state of constitution liable in common to both.[1]

I have observed repeatedly that, during the seasons in which influenza has occurred as a general and severe epidemic, these two diseases of scarlatina and measles have been more than usually frequent; though in no instance, perhaps, reaching the extent to which they occasionally occur at other times;—and further, that during the actual prevalence of influenza, a class of cases has appeared of very singular and ambiguous kind, having many appearances analogous to each of these disorders, but particularly, as I think, to scarlatina. I noticed frequent cases of this nature during the influenza of the spring of 1833, when the scarlet fever was also very prevalent, and produced much mortality in London. A peculiar spotted efflorescence on the skin, generally attended with some angina, and occasionally with slight ulceration of the throat, were the symptoms principally marking this apparent relation; which was sufficient in degree to suggest the question, whether it arose from the concurrent

[1] The remarkable case alluded to was that of Lord ——, who at the age of fifteen had the measles in the ordinary way, caught at a public school, when prevalent there. This was followed about two months afterwards by scarlet fever, with ulcerated throat: which went on in the usual course, leaving behind the most extensive anasarca over the body which I have ever seen in sequel to this disease;—cured by bark and steel, with blue pill and diuretics. Two or three years afterwards, Lord ——, when at Berlin, had the measles a second time. The physician who attended him in that city, entertained no doubt as to the nature of the disorder. And what forms the extraordinary part of the case, this attack of measles was followed again, after the interval of a few weeks, by scarlet fever, unequivocal in its symptoms, according to the information I received—with desquamation, but no anasarca following it. It seems impossible to suppose mere casual coincidence here; and I do not see any equivocal circumstances in the case, unless it be possible that what was deemed measles in each instance was merely an imperfect manifestation of scarlatina, followed by the more regular form of the disease.

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action of two morbid causes; or from one virus capable of producing different forms of disease, according to the texture on which it fell, or other less obvious circumstances.[1] If hazarding an opinion on this obscure subject, it would be in favor of the former view. Though we have many instances of such seeming incongruity of two diseases, that one present in the system precludes the ingress of the other, still there is no foundation for a general law to this effect. On the contrary, we have various proof that morbid actions, derived from sources wholly different, may coexist in the body; severally modifying each other, though under conditions scarcely even surmised in our present knowledge.

Not less worthy of notice are those curiously anomalous cases, which occur in conjunction with scarlatina, when this disease is prevailing alone. In close contact with distinct and violent instances of the disorder, we find others in which the influence of the same virus is manifestly present, but where the symptoms are incompletely evolved; often so partially, as to wear every appearance of other and different disorders, and frequently to obtain wholly different names; yet, amidst all these anomalies, capable by diligent observation of being referred to a common cause. I had occasion in the last chapter to speak on this curious subject of the incomplete development of disease from a virus of given kind, insufficiently received for complete effect, or acting on peculiar temperaments of body.

[1] I have observed also many of these ambiguous cases during the yet more remarkable influenza prevailing while I write this note (Feb. 1837). In one singular instance, I traced something like a separate course of the two diseases in one family; two children having well-marked scarlatina, with ulcerated throat, eruption, and desquamation, in a house filled with cases of influenza. It is worthy of note, that the symptoms of influenza in these children preceded those of scarlet fever by several days; and appeared distinctly to recur in succession to the latter, as a fresh seizure, after an interval of two or three days almost free from disorder. At the same time, two other children of the same family showed the ambiguous symptoms described above, and another child passed from the influenza into a fever, having the typhoid or adynamic type.

Scarlatina was again very prevalent in London in the early part of 1838, simultaneously with a recurrence of epidemic influenza; and I noticed several of the anomalous cases cited above, though less remarkable than those of former years; possibly because the influenza itself was less severe in degree. The same conjunction I have observed in two or three subsequent instances. Sir G. Baker notices the tendency to angina and erratic efflorescence of the skin in the remarkable influenza of 1762.

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Erysipelas and erythematous inflammations are frequently found to prevail during particular seasons, and in certain localities; and with many common characters. Such are the affection of internal membranes, the translation of the inflammation to the skin, the frequent presence of fever of low or typhoid type, and the influence of this state of habit upon all casual injuries or disorders of the body occurring at the time.[1] Instances of this kind were formerly more explicitly recorded, as the writings of Sydenham testify, in connection with the doctrine of an *epidemic constitution* of seasons—a theory which scarcely receives its due regard in our own day.

A further example may be found in the sort of epidemic character which puerperal fever often takes, in seasons when erysipelas is very prevalent; rendering it probable that there are some causes in common to the local and constitutional disease;—if not indeed the same morbid cause to both, modified in its action by the texture affected, and by the individual peculiarities of each case. The presumption, now rendered almost certain, that both puerperal fever, and erysipelas in its ordinary character, become contagious under certain circumstances of type, season, or locality (and these seemingly alike in different instances), may be admitted in further proof of such a relation.[2] And further,

[1] In a very valuable manuscript narrative, by Dr. Hillyar, of the Cholera which prevailed on board the Caledonia, when cruising between Sicily and Malta in the autumn of 1837, I found a curious notice as to the peculiar tendency to Erysipelas in this ship for several successive years: a fact agreeing with what is known to occur in particular hospitals and localities on shore, but more striking here from the still closer limitation of the circumstances. The Caledonia during this time always bore the repute of being a sickly ship; a larger proportion than usual of the crew suffering under any existing epidemic, and with erysipelas generally as a concomitant malady.

For some curious facts bearing on this subject, see Dr. Budd's paper on the cholera as it appeared on board the Dreadnought Hospital Ship, in October, 1837. —*Med. Chirurg. Trans.* vol. xxi. The state of the Agamemnon at Portsmouth in the summer of 1853, was an instance still more remarkable of the accumulation and persistence of pestilential disorders in a single vessel. The subsequent progress of the naval war, both in the Baltic and Black Sea, has furnished many similar examples. Wherever human beings are brought and kept together in close contact—in prisons, barracks, ships, or besieged fortresses—there especially it would seem that the atmosphere becomes fitted for the reception, retention, and propagation of the *fomites* of disease.

[2] This statement has very little to do with the disputes that have existed as to the nature and treatment of puerperal fever or peritonitis; further than in confirming

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we have reason to believe that there is increased tendency to both disorders during the prevalence of epidemic influenza; as was well attested with regard to the puerperal fever in the spring of 1838.

The hooping-cough among children has also been singularly concurrent with some of the epidemics already mentioned; particularly, as I find in my notes, with the influenza, which spread so universally over England in the summer of 1831, immediately before the first appearance of the Asiatic cholera in this country: and again very remarkably with the similar, though less severe epidemic, which prevailed in London during the spring of 1838. The cases were numerous at both these periods, in which, in children especially, it was scarcely possible to draw distinction between the two disorders; and difficult to avoid the persuasion that some cause was concerned, analogous or common to both.

The Infantile remittent fever, in its various forms, has equally prevailed during the period of some of these influenzas; and in such close concurrence as to give strong suspicion of a relation more intimate than that of mere coincidence of time. It may be urged again that there is nothing more in these various cases than a state of season favoring the evolution of distinct disorders, which can coexist, and perhaps blend with each other. But the connection of symptoms between hooping-cough and the infantile fever goes beyond this; and points at some common or similar cause, modified in its effects by acting on different temperaments or different parts of texture. The hooping-cough rarely prevails as an epidemic without being conjoined with great frequency of those bowel disorders among children, attended with remittent fever, to which, in their more marked form, this name of infantile fever has been applied. And, if objection be made that the one disorder is infectious, the other not, it may reasonably be asked whether the latter assumption is certain? whether the evidence of occasional infection in this fever is not as clear as in erysipelas or dysentery? and whether the mode of proof is not the same as for hooping-cough itself? I am led to believe that no such line of distinction can fairly be drawn between the cases in question.

I have often had occasion to notice a fact further illustrative of the subject before us. This is, the singular frequency, espe-

the opinion that at certain times, and under particular circumstances, the inflammation present has a different character from that which exists at another.

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cially among children, of ulcerations or eruptions of the mouth, fauces, nose, lips, and face, at those periods when bowel disorders, whatever their cause, are peculiarly prevalent and severe. The uniformity of the connection, as well as the frequent translations from one form of complaint to the other, leaves no doubt of the relation here; and we must necessarily look to a common cause of irritation or disorder, affecting different portions of a continuous texture; as determined either by idiosyncrasy of the patient, or different application of the cause itself. That the one form does occasionally, under such circumstances of variation, become a complete substitute for the other, I can as little doubt as I do the frequent interchange of these morbid actions, even between the most remote parts of the alimentary canal.

Facts of this nature, it should be remarked, do not subserve to medical theory only, but are capable of much more useful application to practice than is generally given to them. What we can see externally in some cases, may be taken, with certain allowance, as an index to the state of parts concealed from sight. If in a given epidemic we find many children having the throat and mouth beset with irritable sores, it becomes a reasonable suggestion that drastic or frequent purgatives are unfit for the more numerous cases, where the same disorder is shown by irritation, depraved secretions, or even by torpor of the intestinal canal. Mercurial alteratives may be admissible, or even expressly indicated; but other objects of treatment are for the most part best attained by mild remedies and a proper regulation of food; the latter, moreover, being regarded even more as a medicinal agent, than as conveying nutriment to the body. I find this conclusion so generally justified in my own experience, that I cannot hesitate in recommending attention to it.

It is probable as a general fact (and, if true, a very important one), that the disorders already mentioned, incidental chiefly to childhood, have virtual identity with certain fevers or other diseases of adult age; the operation of the causes varying according to circumstances, of which difference of age may readily be admitted as the chief. Dysentery will here suggest itself as one of these kindred disorders. It has various relations to the infantile fever,—in the course of the symptoms, in the textures affected, and in the lesions of structure consequent upon it;—and perhaps no differences beyond what are compatible with a

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common cause, acting under different conditions. It is further worthy of remark, in pursuing this series or circle of relations, that dysenteric symptoms are generally frequent at the same period as the disease just mentioned, as well as during the seasons when the influenza has appeared in its most severe epidemic form. I find it remarked in the notes I made on these epidemics, that the dysenteric tendency, as well as other forms of bowel disorder, becomes much more frequent when the influenza, in its first and more obvious symptoms, might be said to have subsided.[1] It may admit of doubt whether this should be regarded as a translation of morbid action derived from the same original cause, or merely as an indirect and casual effect, to which the system became more liable from the previous disorder. Either supposition may be true; but I think the former, on the whole, the more probable one.

This connection between epidemic dysenteries and pestilential disorders has been noticed by many writers; and numerous instances have been recorded in proof of it. The occurrence of dysentery, in sequel to the latter, has led to an opinion that it may be produced in such cases by morbid exhalations from dead bodies. Even were this view admissible in some instances, it certainly is not so in that of the epidemic influenzas, as we have known them in our own country; and here the dysentery following may be more reasonably attributed to one of the causes stated above.

In referring thus frequently to these Epidemic Influenzas, which have so frequently and widely prevailed of late years, I may add that no class of diseases lays open to us a larger field of practical inquiry; as relates not only to their own nature, but also to the connection of other diseases, with which they are closely associated. In a succeeding chapter (on Epidemic Influenzas) I shall speak more at length on this subject. The simultaneous or rapidly successive influence of a common morbid cause over large communities and countries, discloses relations which in no other way are equally accessible to research. In showing the various forms which a single disease is capable of

[1] It is worthy of remark, that Dysentery occurred in London as a severe and fatal epidemic, in 1762, during the three autumnal months, beginning only a few weeks after the cessation of the fatal influenza of the same year. Sir G. Baker, who has well described both, designates them as "duo morbi memorabiles, qui in eundem annum, hic veris, ille autumni soboles, inciderint."

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assuming, it illustrates the nature and action of the circumstances which thus modify it, and especially the effect of particular textures in altering the aspect of the symptoms. We have not yet sufficiently drawn from this source of knowledge. It is probable that we may hereafter learn from it the virtual identity of many diseases; hitherto placed asunder by distinctions which have foundations only in subordinate symptoms, thereby disguising from us what is most important both in pathology and practice. Or, if no such identity be proved, we may find evidence, scarcely less curious, of an endemic state of constitution (be it called adynamic, or by any other name), which, originating with the same causes that produce the symptoms of influenza, renders the body for a time more prone than usual to certain other disorders, the material causes of which are ever more or less present. Each mode of viewing the subject is probably correct in part, and they are perfectly compatible with one another.

In further illustration of this topic, I may briefly notice the singular analogy to the milder forms of typhus and of intermittent fever, which these epidemics have occasionally presented. It would seem, as I infer from my own observation, that the former, or typhoid fever, prevailed most at the time when the virus of the disorder was in its greatest activity. There is cause, also, to believe, from the history of the disease in its whole course and spread, that this type was more common in some localities than others. It is further certain that some constitutions have peculiar liability to be thus affected. The tendency to intermittent symptoms—both tertian and quotidian in type, and often very regular in period—seemed to me more common when the disorder was abating; and here too it was manifest that individual habit had much to do in determining the nature of the symptoms taking this character.[1]

The Typhus fever itself, in the graver form which we now rarely see in English practice, as well as in those milder shapes to which such various names are applied, has its probable connection with other diseases; and especially, as some recent authors have supposed, with the contagious exanthemata or

[1] Sir G. Baker describes the same intermittent type in the fevers attending the influenza of 1762.

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eruptive fever. That the relation here is beyond that of mere casualty can scarcely be doubted. Whether inquiry begin with the exanthematous disorder, or with pure typhus, equally are we carried into certain common symptoms, sufficiently close in character, and frequent in occurrence, to justify the belief of some community of origin. From the true petechial eruptions, which have at all times marked more or less the severe forms of epidemic typhus, to the slight and fading exanthematous spots of common epidemic fevers, we have every grade by which to connect the eruptive tendency with the other symptoms most essential to the character of these diseases. We can scarcely, indeed, touch upon this subject of fever (particularly that which our present knowledge obliges us to consider of idiopathic kind), without finding in it a bond by which to associate together numerous forms of disease; but withal a knot so intricate, that no research has hitherto succeeded in unravelling it.

Looking to the great influence of the several conditions of idiosyncrasy, season, and locality, in producing varieties of disorder from a common cause, as in the instances I have stated, it is impossible to doubt that the same principle of inquiry might be carried much further into the history of disease. Other conditions may be added, to which I have already shortly alluded; such especially as the varying intensity of any given virus, and its action upon different textures in different persons, or upon the same texture in different parts of the body. That all these modifying causes exist is certain on the most superficial view of the history of disease. Their influence in determining its course and aspects is no less distinct than important. In the influenza, as it prevailed successively in 1831, 1833, 1836, and 1837, the action of the virus was chiefly upon the mucous membranes of the respiratory passages and alimentary canal; the symptoms

[1] Dr. Roupell has described a form of fever which prevailed in the Grampus Hospital Ship, in 1831, under the name of the Febris Typhodes Rubeoloida, from its being accompanied in almost every case by a rash like that of measles. A similar fever, though somewhat less marked in character, was very frequent in London a few weeks after the greatest violence of the influenza in 1837; attended in numerous cases with the same anomalous eruption. Again, during the early months of 1838, and concurrently with, as well as subsequently to, the influenza then prevailing in London, the same character of low or adynamic fever existed to considerable extent and fatality; attended with eruption, sometimes of scarlet, more frequently of dusky red spots. Instances of this nature are very frequent in medical history, though not always related in their proper connection.

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varying according to the various local direction of this morbid cause. It appeared to me certain in evidence, not merely that the virus affected primarily a different portion of these membranes in different persons; but also that many of the phenomena of the disease, in its progress and sequelæ, depended on the removal of this action from one part to another.[1]

In variations such as these, we have the probable index to relations of disease of a simpler and more elementary kind, than those upon which our present systems of Nosology are founded. For while admitting, as must be done, that the causes just cited do greatly modify the aspects of a disease in individual cases (still leaving assurance of its identity with some epidemic prevailing at the time), is it not possible, or even probable, that a given material cause of disease may occasionally be so far modified in action by causes more largely operative, whether atmospheric or otherwise, as to assume the aspect of another epidemic, and to obtain separate name as such? This is the furthest point to which the speculation can be carried;—not sanctioned indeed to this extent by any direct evidence (for in our ignorance of the material causes it is very difficult to procure proof), yet plausible enough to warrant its being entertained among the many questions open to research.

One step towards future success in these inquiries will be the relinquishment of the too exclusive attention hitherto given, even by physicians themselves, to the eruptive part of certain maladies; an error which has naturally arisen from the obvious nature of the symptom, and the more ready diagnosis it affords. I allude here especially to the contagious exanthemata: in which diseases, the common facts of a virus received—of its maturation for a certain time within the system—of symptomatic fever—and of the translation of morbid action from internal to external parts—though far from proving identity of cause, yet indicate a series of similar morbid changes, and actions analogous in their nature and progress, amongst the most important in the whole history of disease. The eruption here is not itself the disorder; but one only, and that not always the most critical, of the series of changes composing it. Every one who has noted the variety

[1] In the much milder epidemic of the spring of the present year (1840), the symptoms affecting the membranes of the alimentary canal might be considered predominant over others.

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of anomalous symptoms which arise from scarlet fever imperfectly developed, or the disorders which often precede by a considerable time the appearance of erysipelas on the surface, will at once recognize the truth of this statement. It may seem needless to draw attention to these circumstances: and yet it is not wholly so. The influence of names and of early opinions is not easily overcome; and here, as elsewhere, they have effect in withdrawing us from that method of pursuit which is best fitted to lead to further knowledge.

In writing on this subject, I might refer to many eminent authors who have touched upon it in parts; and more especially in what regards the relation of fevers and pestilential or epidemic diseases. The foregoing remarks indeed form a mere outline of one portion of the inquiry, and must be considered as such. But it is that portion which is the most remarkable in itself; and which by its relation to idiopathic fevers, and to the general principle of contagion, establishes the largest and most intimate association with diseases of every other class, and particularly with those depending on morbid states of the blood.

I may repeat here the remark with which I prefaced this chapter, that the assured result of future inquiry will be to associate together, by common physical relations, diseases which have hitherto had separate, and even remote, places in our systems of Nosology. Of the various systems of classification attempted for the record of disease, none can be rightly considered otherwise than as provisional; none certainly approach to the fulfilment of those conditions which are essential to the completeness of such a work. This is no matter of reproach to those who have made the attempt; but an inevitable result of the difficulties which beset it, and which no single effort can overcome, even if time and combination should ever succeed in doing so. I am induced to add here a few general remarks on the subject, because though the path through such systems is a thorny and entangled one, it must needs be traversed in some part by every student of medicine; and it is well to know what the hindrances are, for the better chance of their being overcome. I have further motive for these remarks, as illustrating the method pursued not only in this, but in various other chapters of the present volume, of bringing together, under some common principle, or analogy of causes, and morbid conditions, diseases hitherto generally disso-

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ciated in our nosological systems;—such separation being founded on other analogies, more obvious perhaps, but often much less essential as pathological phenomena.

The main objects of all classification and systematic nomenclature of diseases are,—facility of teaching;—the means of intelligible and precise communication among medical men, including those of different countries and ages;—and the technical aids afforded to the practitioner himself, with which, whatever his ability, no one can wholly dispense. The principles are mainly the same which apply to these several objects. The methods which afford greatest facility and exactness to the communication with others, are best adapted to give right guidance in practice; and, with certain adaptations, to the office of teaching also.

To meet these demands, whether thus or otherwise defined, much labor, learning, and ingenuity have been employed. It is needless to name the many eminent men who have engaged in the work. To examine critically the systems they have proposed would require a volume in itself. It is a case, however, in which both the difficulties of original labor and those of criticism find recompense in the deep interest of the subject. The questions of evidence to be settled; the fixing of connections and differences; the separation of what is subordinate or incidental from the primary and fixed characters of disease; and the reconcilement of anomalies; might approve themselves as an exercise of the understanding, even were there less assurance of value in the results. The difficulties are such as to make a science of method alone. Though many occur of similar kind in other branches of knowledge, in none probably are they of equal amount.

Of the systems of Nosology hitherto proposed, each has its peculiar excellencies and defaults; and it is not paradoxical to say that these have close relation to each other. Each leans to some particular principle, real or hypothetical; the adoption of which has perchance depended on the intellectual bias of its author. Such is still the state of medical science, that any single principle of arrangement, however successful in relation to certain classes of diseases, will be found wanting when applied to others; the seeming completeness in the first case being gained at the expense of some notable imperfection in the

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second. There are no such natural and stable connections here as subsist among the objects of other sciences. The single consideration that we are seeking to classify not natural and normal phenomena, but the disorders and deviations from them, is itself a sufficient index to the difficulties of the attempt. I cannot better illustrate them than by stating that for the varieties of puerperal fever alone, at least ten different forms of arrangement have been proposed, with a different nomenclature for each.

It is manifest, for these and other reasons, that a system of Nosology may fail by aiming at greater exactitude and minuteness than our actual knowledge will allow. And it may fail, not merely as a scientific arrangement, from what Bacon terms "an over-early and peremptory reduction into acts and methods," but also in relation to the practical purposes already mentioned, and which must ever be taken as a test of excellence. Neither labor nor learning are beneficially bestowed if those objects remain unfulfilled; and they are even hurtful, if employed in giving a show of science and fixed relations where the reality does not exist. Though there are some men whose quick and active perceptions, aided by opportunity, speedily correct the errors of early instruction, there are many others who never escape from the narrow circle of system in which they have been schooled. Some technical arrangement is necessary for the purposes of medical teaching; but under the present incompleteness of our knowledge, practical ability as well as the interests of science requires that every system of classification should be regarded as tentative and provisional, and taught as such. Feeble understandings, or minds so constituted as to see science in methods and names only, may be discouraged by this provisional instruction. But the genius best fitted hereafter to redeem its deficiencies, will not be deterred by finding in the outset that there is a field thus largely open for future labor.

What has been said of classification, applies by obvious parity of reasoning to the nomenclature of disease. Medical knowledge at this time is scarcely more capable of being submitted to a technical system of this kind, than was chemistry forty years ago; and any successful attempt to impose it would be equally liable to future change and confusion. But no such success is yet possible; and the nomenclature now chiefly in use does in some

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respects work better even from its imperfections. Uniformity of plan is perpetually sacrificed to the convenience of old and familiar appellation; and though aid has been largely sought for in that prolific language which "gives a soul to objects of sense, and body to the abstractions of philosophy," yet where this has been carried into details, as in some of the systems proposed, the uniformity of the principle does not prevent the result from being useless. Works thus elaborate in nomenclature have their places on the shelves of libraries, but never in the daily and hourly uses of practice.

Illustrations of the foregoing remarks may be found in every part of pathology, and especially in all that relates to the doctrine of fever under its various forms. The effect of the difficulties described is denoted by the fact, that hitherto no system of Nosology has approached to general adoption, in this or any other country. That of Cullen, which, with all its deficiencies, does honor to the memory of this eminent physician, has for various reasons found best reception in our own schools. But its influence as a basis or method of medical doctrine is continually giving way before those various discoveries in physiology and morbid anatomy, which in the new relations they disclose, have modified every part and principle of pathological classification.

Meanwhile, and in face of all these difficulties, admission must again be made of the need of some such provisional system; and the principle upon which it may best be based is fairly open to inquiry. Classification may be sought for, either in the particular character of the morbid actions which produce disease, or in the organs and functions themselves, viewing them as severally subjected to these morbid actions. Each of these general principles, again, admits of being differently treated;—the first, according to the various modes of regarding the actions of disease;—the second, according to the manner of arranging the functions, and the organs connected with them. Examples in illustration are readily afforded by the nosological systems most familiar in reputation or use. Such examination, however, will show these principles of classification to be often incongruously blended together, as well in systems as in the extemporary methods of practice; both being natural results of those complex relations of which I have before spoken, and which render uniformity of method almost impossible to our present knowledge.

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CHAPTER VI.

ON DISTURBED BALANCE OF CIRCULATION, AND METASTASIS OF DISEASE.

I PLACE these subjects together, not merely for mutual illustration, but because in some points they can scarcely be regarded apart, when looking to the phenomena of disease. I have often had occasion—and very especially in the last chapter—to speak of the advantages of considering such phenomena in different relations to one another, detached from those technical arrangements upon which our systems of nosology and nomenclature are chiefly founded. Here is another instance where much may be added to the extent and clearness of our views in pathology, by considering different disorders in reference to one common principle of morbid action. We thereby, even without aiming at any new classification, gain knowledge of connections, which escape notice if confining ourselves to a single view, or to the limits of systematic nomenclature; and we obtain moreover various aids in practice, made more secure by resting on a wider basis. Methods of inquiry are reasonably multiplied, where truth is so often reached through relations the least obvious to common observation.

Disorder or irregularity in the circulation of the blood, by whatsoever names in its different forms described, may be termed in this sense a principle of morbid action;—more universal perhaps, and more important in its effects, than any other in the animal economy. Taking the expression, indeed, in its largest sense, we may affirm it to involve every possible form of disease. Scarcely one can be named, in which some unequal balance of blood, local or general, does not exist. From acute local inflammation of the most urgent kind, to chronic inflammation, plethora, or congestion, under the slightest forms; and again, to those momentary and ever-changing misdirections of blood which occur

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in nervous disorders, or arise from the thousand casual causes disturbing the economy of life—we have a long series of morbid changes, in all of which this common fact is concerned—however various the causes of inequality, and however different, or even opposite, the state of the vascular system in the changes taking place.

It is further to be remarked, that all these morbid actions have connection, more or less intimate, with corresponding changes in the healthy state; and that they become abnormal chiefly by excess in amount or duration. The inequalities in circulation produced by mental emotion; by bodily exertion or posture; by digestion in its several stages; by exposure to heat or cold, or other atmospheric changes; represent in degree, or for a time, almost all such as are considered morbid in kind, and often pass into the latter without any obvious interval between. Such manner of viewing the subject, through the relation of disordered actions to those of health, is here as elsewhere, the best we can adopt, whether we look to the theory of disease, or to its treatment in daily practice,—a remark I never scruple to repeat under the opinion I hold of the value of this method of inquiry.

It is difficult, from the nature of the subject, to classify the various disturbances in the balance of circulation. The most general distinction might be that between inflammation, in the common acceptation of the term, and those irregularities, often much more extensive and sudden, but where none of the characteristic marks of inflammation are present. Here, however, besides many exceptions and doubtful cases, we come upon the wider question, whether there are any distinctive characters of inflammation, certain enough to warrant their being thus classed apart. This doubt reasonably arises as a result of all modern inquiry on the subject. Though it may be difficult to go so far with one eminent pathologist, as to remove the term *inflammation* from medical use, yet it is certain that the gradation in all these states is such, as not to allow any fixed lines of demarcation to be drawn, without error in fact, and frequent injury in practice.[1]

[1] All who have read on this subject, as well as studied it in actual practice, will be conscious of its many perplexities; and of the frequent impossibility of diagnosis between inflammation and simple congestion, or the more singular phenomena of turgescence. As an example of these difficulties, we might specify some of those curious instances, in which a state, having many of the received characters of inflammation, is actually brought on by causes of depletion, such as increase

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Still the pathological differences for each end of the scale are so strongly marked, as fully to justify their separate consideration; and though the notion of metastasis, as commonly received, applies to both classes of phenomena, there is convenience here also in keeping the general distinction constantly in view.

So much has been written upon inflammation, that it is not easy to add either fact or opinion to those already before us. But the other inequalities of circulation, though more considered of late years, have not yet received the same minute attention. From the difference of the phenomena, research is here more difficult; and on many points we are limited at present to facts drawn from other parts of the animal economy. This very connection, however, is matter of great interest to pathology; and in all points of view these unequal distributions of blood, which, without local inflammation, constitute so many forms of active disorder, deserve the diligent attention of the physician.

The most general manner of viewing the subject regards the causes and conditions under which the blood is thus unequally transferred from one place to another, producing local excess or deficiency, as the case may be. It is clear that there is a natural balance, or proportion, of blood belonging to the several parts of the body respectively; certain, and often great, deviations from which are compatible with health, or even essential to the functions of life; while yet greater deviations, or such as affect particular organs, or occur too suddenly, fall strictly within the name and character of disease. What are the physical causes producing these changes? Why is it, that even in healthy actions, and more remarkably in those of disease, mutual exchange of blood should take place, by sudden determination, between the skin and internal membranes? or why those more

the proportion of serum to the coagulum of the blood; and is relieved by opposite treatment. Other anomalies might be named, familiar to every practitioner. Some of these may best be explained by looking less than is usually done to the mere vascular tissue of an inflamed part, and more to the state of the fluid within the vessels, as primarily affected by the causes of inflammation, and determining the changes which ensue, both in the part itself, and the system at large. An experimental memoir by Dr. Alison, read at the meeting of the British Association at Dublin (see *Report of Sections*, p. 88), has great value in relation to this topic.

The opinion, advocated by Dr. Macartney, that inflammation can never be rightly considered a reparative process, is another proof of the uncertainty which still hangs over every part of this subject.

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vague and various translations of blood which occur among different organs, without apparent unity of cause;—as the brain; the membranes lining the air-passages; the pleura and lungs; the peritoneum and membranes of the alimentary canal; the liver; the kidneys; the organs of generation; the mammæ; the synovial membranes? &c. That there are parts of the body more closely connected than others as respects these translations, and that there is some relation here depending on similarity of texture and community of function, is certain from observation. But even could we derive any direct explanation from this, the facts would call upon us to go beyond it; and to show cause for those sudden changes in the balance and direction of the blood, where the structure and function of the parts thus successively affected are wholly different in kind.

Or it may be needful to enlarge the inquiry, to include the cases where, without actual increase or diminution in the quantity of blood in an organ, the rate of its motion is topically altered, so as to change for a time the condition of the part. It is not easy, indeed, to prove that such case exists. If it does (and perhaps the circulation through the brain furnishes the most probable instance), it comes within the scope of the subject, and receives illustration from the remarks which follow.

Four conditions at least may be taken into question, as respects the rationale of these changes. First, the variations in the heart's action:—Secondly, the state of the capillaries of the parts to which the blood is for the time directed:—Thirdly, the quality or quantity of the blood itself:—Fourthly, the influence of the nervous power upon the circulation. These conditions are in many ways closely blended together; yet each is distinct enough to deserve separate consideration.

The first is by no means the most important, though on superficial view it might seem to be so. By whatever influence the action of the heart is produced (and the question, as regards the nervous system, has scarcely yet been decided), its immediate effect in propelling the blood towards the extreme vessels is one mechanical in kind; nor have we any certain proof that the arteries aid in this transmission by any power which they themselves possess. The action of the heart may be excessive or deficient in force: it may be disordered in frequency, regularity,

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or other characters of pulse; but still these inequalities affect more or less all parts of the vascular system; depending chiefly on the greater or less power of propelling blood into the minute vessels. The general inference remains the same, whatever opinion we adopt upon the questions still perplexing physiologists in respect to the circulation :—whether there be other motive power than that of the heart?—whether the arteries or capillaries have any vital contractile force aiding in this effect? —or whether the vital properties of the blood itself may assist or modify its passage through the extreme vessels? Under any of these views, it is equally certain that the agency of the heart (to which, in the higher classes of animals, all other motive powers are subordinate) must have the same general ratio to each part of the body, whatever the changes occurring in its own rate or vigor of action.[1]

As respects, again, the quality of the blood, and any influence the variations of the heart's action may have in altering this, though we cannot affirm it to be wholly unaffected, and might even suppose some changes to be made by the rate of motion, yet have we no certain evidence of such alteration; and the general presumption undoubtedly is that it reaches the extreme arteries little, if at all, changed in any essential character.

Nevertheless, the variations in the action of the heart cannot be wholly limited in their effect to changes in the general rate and diffusion of the blood. A different propulsion from the central organ must somewhat alter the proportion in which it reaches parts more or less remote; and even the same proportion of excess or deficiency will produce different effects according to the texture and function of particular organs,—a point not sufficiently adverted to in the consideration of this subject. A given excess, for instance, as applied to the liver and kidneys, may have little influence on the system at large; while the same increased action through the brain, or even through some particular portion of it, may not only affect this organ itself, but also produce disturbances of circulation throughout other and distant parts of the body. This, however, is a secondary effect, taking place through nervous agency. The action of the heart cannot alone,

[1] In descending through the scale of organized life, the functions of the capillary vessels in circulation become gradually of greater importance; till, arriving at vegetable life, we find them solely effective in this part of the economy of plants.

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And to this part of the system (a view for which we
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te and complex mechanisms by which, in spaces barely ac-
ible to the nicest instruments, the various functions of
sorption, secretion, and exudation, as well as the translation
of blood from arteries to veins, are all simultaneously going on. What the immensity of creation is to the astronomer or geologist, such are these infinitely small dimensions of matter and motion in space to the physiologist. Presuming, or knowing, that all organization, however minute, such as the many thousand lenses which compose an insect's eye, must be due to the action of distinct vessels, circulating, secreting, and absorbing, we have some vague measure of that exquisite minuteness of fabric and formative action, upon which life in its several parts essentially depends.

The discoveries of Dutrochet and others prove indeed that many of these changes may and do proceed, without the intervention of any continuous vascular structure; and that we need no longer look for the open mouths of vessels as essential to the functions by which one fluid is separated from another. It is still matter of dispute among physiologists whether the capillaries are really membranous tubes, or merely interspaces of tissue, through which the blood finds its passage. But though certain of these minute actions may be submitted to a common physical principle, it is obvious that we must admit essential differences of structure or nervous power, or both, to explain their actual

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diversities, and the relation of one common fluid to these separate functions. This is obviously true as regards the glandular texture; and even in the instance of the serous membranes, where the tissue appears so much more uniform and simple, the secretions from different membranes of this class vary in the proportion of water, animal, and saline matters they contain; and this more remarkably under the respective conditions of health and disease.[1]

It is further manifest, and now generally recognized, that changes take place in these extreme vessels, or in the tissues of which they form the largest part, independent altogether of any previous alteration in the heart's action, and often connected with the latter only as an occasional cause of disturbance to it.[2] This is a point of great importance to the particular subject before us, and in truth to every part of medical science. The capillary vessels, as they are the seat and source of many of the great actions of life in a healthy state, so have they like relation to disease in all its obvious forms. Here occur, as far as we can observe, the first morbid changes in fever and inflammation; and there is scarcely a symptom in the progress of such disorders, in which their functions, in one part or other, are not involved. The changes they undergo in all chronic diseases, either are originally, or become, an essential part of the malady. Vitiated states of the blood are testified in similar way; and through this part of the system morbid deposits and absorptions take place, and all the diversities of organic disease are more immediately produced.

The direct relation of the capillaries to the sensorium and nervous system is shown in various ways, which will afterwards be mentioned. At present we have only to notice this part of

[1] Müller's admirable treatise on the Structure of Glands, as well as the later researches in his great work on Physiology, may be considered to have settled several principles in the general doctrine of secretion, particularly those which regard the relation of vascular structures to the effects they respectively produce.

It would be impossible to refer to all the questions that exist respecting the capillary system, and to the researches (through the microscope, injection, and other means) which have been directed to their solution by the most eminent physiologists of the day. They are indeed among the most important in physiology, as approaching nearest to those ultimate actions on which depend all the functions of life.

[2] The experiments of Dr. Philip, Le Gallois, and Serres, have shown that particular injuries of the brain and spinal marrow are capable of altering, or in some cases wholly arresting, the circulation through the capillary arteries.

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the circulation in its connection with the sentient extremities of the nerves; the sensations derived from which are variously altered by the quantity and manner of transmission of blood through the extreme vessels. But beyond this, we must probably look to the capillaries as the first seat of those vital actions which are occasional to the system; as the enlargement of the uterus and its appendages during pregnancy; the swelling of the breasts and secretion of milk afterwards; and others which are too familiar to need recital. Any changes in the centre, or larger branches of the vascular system, are undoubtedly subordinate to those which have their locality in the very organs thus determined to particular actions of life.[1]

Another important relation of the capillaries, requiring notice, is that to the external atmosphere, particularly to the fluctuations of heat and cold. I have spoken on this subject when treating of the influence of weather on the conditions of disease. It is manifestly through the capillaries of the surface that these external causes chiefly act on the body. Their actual effect in disturbing the circulation, and altering the balance between outward and inner parts, is shown in numerous cases, both in health and disease; to which, from their familiarity, it is merely necessary to allude as coming under this part of the subject.

Seeing, then, the numerous and remarkable functions of the capillaries throughout the body, it is certain that we must seek here the source, as well as evidence, of many of those disturbances in the balance of circulation which form the subject of this chapter. Altered or disordered action may be induced in the extreme vessels of any part by causes affecting the whole vascular system; but it often originates in them locally, and becomes thence a source of disturbance to the central organs. And this not merely where a particular function is to be performed, as in the cases just quoted, or in the more frequent and familiar cases where local irritation is applied; but also from other conditions less cognizable within the part itself, altering the quantity and rate of blood circulating in it, and thereby changing, more or less in degree, its relations to the rest of the system.

[1] The singular case recorded by Dr. Houston, of an acardiac twin fœtus, shows that the capillaries may maintain in some degree, through their proper actions, the general movement of the circulation; one question, amongst others, which has created controversy on the subject.

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Though discrimination of the cases is not always easy, yet multiplied instances might be given (and would be well worthy of collection), where changes take place in the capillary circulation of a part, having no connection with the heart or general circulation, except in the effects they actually produce by reflected action upon them. Certain forms of metastasis which I shall afterwards mention, furnish examples to this effect. One curious illustration, valuable in its generality, may be found in the fact (not much noted, though apparently unequivocal) that each stage of the simple febrile paroxysm may be represented locally in detached parts of the body, without obvious affection of the system at large. To a certain extent these partial effects happen even in the regular ague, especially when the paroxysms are slight. But the instances are more explicit, for our present purpose, of transient rigors, or flushes of heat, or sudden sweats, occurring on limited parts of the surface of the body, and disappearing where they have arisen.[1] I cannot state, on observation, that these circumstances do actually succeed each other in series; but in some cases it probably is so; and at all events the fact that they may happen singly, and in this limited manner, is interesting in relation to a part of pathology, as important as it has hitherto been obscure.

Admitting, then, the general position that disturbances of the circulation may begin from the extreme vessels, and tend towards the centre, the question remains—one of the most subtle and difficult in physiology—to what agency these phenomena are due, which thus have local determination without apparent local cause; and which so frequently, rapidly, and unexpectedly shift themselves from one place to another. In stating the third and

[1] The rigors and heat are here taken as proofs of the changes of capillary circulation in the part, and this inference is justified by all we at present know on the subject. If heat, especially, be not a consequence of increased influx of blood into the extreme vessels, it is at all events so uniform a concomitant, that the connection cannot admit of doubt. Topical changes of temperature, occurring suddenly from local irritation, make it evident that the immediate cause of this change is resident in the part. Still, unless it be shown that the local temperature, thus created, is above that of a part of the body, the vessels of which, in natural state, are equally full of blood as those of the irritated organ, it does not prove more than that increase of circulating blood brings increased temperature with it; and the general fact of proportion between the heat developed by an animal, and the quantity of its respiration, remains undiminished in evidence. The researches of Chossat, on the influence of the nervous system upon animal heat, have much interest here.

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fourth conditions of the inquiry, I have already referred to what seem the only intelligible sources of such effects; viz., differences in the blood brought to the part affected; or the influence of the nervous power in giving this direction. Any changes in the vessels themselves, either as regards elasticity or muscular power, must be considered secondary to one or other of these causes. They cannot be conceived as initial actions, though they may modify such as have begun.

The tendency, indeed, of recent inquiry has been to show that while the mechanical propulsion from the heart is the great cause carrying the blood through its whole circuit, there are various and ever-changing actions going on in the extreme vessels which check, accelerate, or otherwise modify its movement, besides affecting those other remarkable changes which belong to this part of the system. And other observations seem to warrant the inference, already noticed, that these actions are less due to the living solids—to any direct contractions or relaxations of the vascular tissues—than to vital changes in the blood itself, in passing through these minute vessels. This view of the general relation of the solid and fluid parts in the capillary system (though not necessarily involving the assumption of a self-propelling power in the blood) is still the subject of much controversy. The result must be regarded as one of equal interest to physiology and pathology, and bearing directly, it will be seen, on the subject before us.[1]

We come then here to the third part of the inquiry, viz., the influence of changes in the quality or quantity of the blood on different parts of the circulating and secreting system;—a wide subject, on which physiologists have hitherto obtained only insulated or imperfect facts. The difficulties, in fact, are great; involving on the one side the most minute structure of solid parts, only partially known to us in their intimate functions;—on the other, the numerous and ever-changing conditions of a fluid, forming a very large proportion of the total weight of the body—singularly complex as a chemical compound—possessed

[1] Magendie, Müller, Treviranus, Baumgärtner, Dutrochet, &c., may be cited among the continental physiologists who have chiefly engaged in this discussion. The two first-named strongly concur in negativing the idea of any spontaneous movement, or self-propelling power, in the blood.

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of vital properties which modify all its others—incessantly altered by addition or subtraction of some of its many ingredients—and liable to the graver and more permanent alterations of disease. Incomplete, however, as our knowledge yet is on these topics, we have proof enough to assure us that the quality as well as quantity of blood, must have effect, more or less, on every part of the circulation. There is every reason to consider the heart subject to this influence, in addition to that derived directly from the nerves; and the modifications of its action thence arising are clearly of great moment in the animal economy.[1] The trunks of the arterial and venous systems may not have other relation to the composition of the blood than as regards its varying degrees of fluidity. The capillary and secreting vessels are those where this influence reaches its maximum, and where the proofs are most numerous and distinct; especially, as might be inferred, under the actions of disease.

We have further reason for believing, though equally without knowledge of the proximate cause, that the vascular tissue of different organs is differently affected by given changes in the quality of the blood; a difference attested by changes in the functions in the several parts, and in the nervous sensibility appropriate to each. The ratio and order of the two effects are but little understood; and it is safer, perhaps, not to seek to distinguish them until better acquainted with the nature of these ultimate actions throughout the system. The attainment of such knowledge would probably show us the brain as the organ most remarkably affected by them. The alterations in the blood, of which we have evidence in the greater vessels, must be presumed to produce effects on the minute vascular tissue of this organ and the spinal marrow (especially, perhaps, on the cineritious part), of singular importance to the whole economy. Many problems in pathology, hitherto unsolved, may eventually become so, by looking to this source of morbid action.

Taking the simplest view of the argument, it stands thus:—That as the blood reaches the capillaries and secreting vessels of every organ and part of the body in the same state as to

[1] The observations which seem to show that certain variations in the sound of the blood passing through the heart depend on differences of its composition, have value as evidence here. Several instances have occurred to me where there was cause to suspect this to be the case.

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quality, whatever this be;—and as these several parts, from essential differences of tissue, vascular or otherwise, are capable of modifying differently the passage of the blood and the products therefrom in the healthy state;—so is it to be presumed that every change of quality in the blood will produce different effects according to these respective varieties of texture. One part may be little affected by alterations which will excite great disturbances in another. We can scarcely entertain any other view than this; looking at that delicate balance in the composition of healthy blood which, whether it be cause or effect of its proper vitality, has manifestly relations so important to every organ of the body.

This manner of regarding the subject illustrates the facts both of health and disease more consistently than any other. It recognizes that appropriate vitality, vascular irritability, and nervous power of every part, which we cannot doubt to exist; and associates the peculiar functions of each with the changes going on in the system at large. Under disease particularly, this regard to the specialty of organic structure is essential to a right interpretation of the phenomena.

It is unnecessary here to refer to the particular changes in the quality of the blood—whether altered proportion of its natural ingredients, or the addition of new elements—thus variously influencing the tissues to which it is sent. All we require is to show generally that alterations of quality must have effect on the circulation through particular parts or organs; and in consequence may become a source of disorder to the general balance of circulation in the body. This is a topic on which it might be easy to dwell at great length, seeing the various knowledge acquired of late years respecting both the healthy constitution of blood and the changes it undergoes in disease; the latter sufficient in amount to justify the term of "lesions of the blood," and including some of the most definite and remarkable disorders which affect the human frame. In no way can the importance of these changes be better appreciated, than by considering the absolute quantity of blood circulating in the body, and the large proportion it bears to the solid tissues. We look upon the pint of blood just drawn from the arm; and perchance feel wonder, as well may be, at the changes it instantly undergoes. Few, however, carry their thoughts further to the very small

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part it forms of the mass of this wonderful fluid; ever in motion and change, and ministering to every organ and function of animal life.

Without referring to ambiguous or disputed points in the physiology of the blood, we have evidence more than sufficient to assure us that the different degrees of its fluidity; the varying proportion of its serous and coagulable parts, and of the red globules; the several degrees of arterialization; the retention of the proper matters of the several secretions; or the admixture of foreign ingredients; must all have influence more or less on the movements through the vascular system.[1] Looking to the principle of coagulation alone, its vital importance in every office of the blood, and the alterations it undergoes even in the living vessels, we have here a powerful cause of change pervading the circulation in every part, and producing effect on every solid tissue and fluid secretion throughout the body. It may be admitted as a general presumption, that each change in the quality of the blood must alter some of the secretions, and every alteration of secretion change more or less the quality of this fluid. The term "vital affinities" is one well warranted by the course and results of modern discovery.[2]

[1] The numerous experiments made of late years by the introduction of foreign substances, poisonous or otherwise, into the blood, are well deserving of note. All conclusions from them, however, are rendered ambiguous by the fact that although the effects from some are more speedy and violent than from others, noxious or fatal effects are derived from almost all;—showing in this how definite is the vital constitution of healthy blood, and how little capable of being safely interfered with by any direct admixture of new ingredients. It is justly remarked by Liebig, that the chemical force and vital principle in the blood hold each other in such perfect equilibrium in the blood, that every disturbance, however trifling, effects a change in its nature. The conclusions from this fact extend themselves widely over every part of pathology.

[2] Though not adopting the bold phrase of Bordieu, "*Le sang est de la chair coulante*," we must admit that the further we carry research into the capillary circulation, the closer are the relations established between the solids and fluids of the body. No author has treated this subject more ably than Andral, in the first volume of his Anatomie Pathologique. I must refer also to the Lectures on the Blood and its Diseases, by M. Magendie; which display the same originality and boldness as the other labors of this physiologist. On the subject of the peculiar affinities of vital action, we owe much to the researches of Tiedemann, Prout, and Raspail. The more recent views of Professor Schulz of Berlin as to the organic elements and vital constitution of blood are yet too hypothetical to be admitted as a part of physiological science.

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The views just stated regarding the influence of the quality of blood on its manner of circulation, apply to its variations in quantity also. These must mainly have effect, as in the former case, on the action of the heart, and on the movements through every part of the capillary system. The frequency and vigor of the heart's contractions clearly depend in part on the varying supply of blood to its cavities; and there may be fault either in the excess or deficiency of this; as is familiarly proved in the effects resulting respectively from undue plethora, or from excessive hemorrhage. In neither case are its actions rightly performed; and the influence of the disturbance, existing in every form and degree, is felt in some proportion throughout all parts of the body. Notwithstanding the frequent occasion for noticing them, and their great importance in the treatment of disease, these effects are still not sufficiently regarded in general practice. Oppression of the heart by quantity of blood, beyond its power of ready propulsion, is sometimes mistaken for debility: and those singular disturbances to the nervous system which occasionally occur from great deficiency or sudden loss of blood, and which have reflected action in disturbing the heart, are often wrongly interpreted, and made a motive for additional depletion. I have alluded to this subject in other chapters; but its importance well warrants the further reference to it in this place.

The varying pressure on their parietes may modify the passage of the blood, even through the larger vessels; but the effect of quantity is doubtless felt chiefly in the capillaries; and from the same causes, already fully adverted to, which render this part of the vascular system the seat of the most important changes taking place in the circulation. We have various testimony of the fact in the altered sensations, secretions, and other actions so produced;—in the many forms of disorder, inflammatory, hemorrhagic, or otherwise, well known as the effects of this cause;—and in the irregular movements and sudden translations of blood, which are prone to occur, both when the vessels are overcharged, and when wanting of their proper fulness and distension.[1] The effects proceeding from the latter cause (the *anæmia* of some physiologists) have, until lately, been less studied than they deserve to be. Their influence upon the brain through the

[1] Many of these questions are ably treated in an Essay on Congestion, Inflammation, and Hemorrhage, by Professor Naumann of Bonn.

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action of the heart and larger vessels has just been noticed. It is probably, like that of the quality of the blood, most intimately felt in the capillary circulation of the nervous centres, and thence becomes indirectly a source of disturbance to other parts and functions of the system.

In considering the latter fact, which bears directly on the present inquiry, we must still keep in mind the respective endowments of each part (whether called vital or by any other name), rendering them differently susceptible to all given causes of change; liable to be acted upon in various order; and in many cases apt to undergo sudden fluctuations in the blood circulating through them. These expressions, though vague, do nevertheless characterize a very important fact in the bodily economy. And they lead us immediately to the last division proposed in this inquiry; viz., the influence of the nervous power in altering or disturbing the balance of circulation.

This very curious topic may be considered in several lights; all, it must be confessed, partaking of the imperfection of our present knowledge; but, in their combination, authorizing conclusions of much importance to this part of pathology. It is certain, from sufficient evidence, that the irregularities of circulation, as well as its healthy state, depends in various ways on the nervous system. The question before us regards the nature and direction of this influence; and it is embarrassed in many parts by the difficulty of distinguishing what is cause, and what effect, where the actions and reactions are so intimate, and exercised in modes so inaccessible to our view.

The simplest and most familiar fact here is the influence of mental emotions in disturbing the balance of circulation;—one nevertheless so striking in its manner of occurrence, as even to have produced theories, which give a sort of locality to the passions in the organs thus affected. Dismissing all hypothesis, the result defined by observation is, that these states of mind act immediately, and without any intervention of will, on different organs of the body;—the action doubtless taking place through the nervous system, in some one or other of its parts; but testified chiefly by changes in the vascular system and altered distribution of blood. These effects, whether of congestion or increased action, are varied by the nature of the emotion; still

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more perhaps by difference of temperament. The heart is obviously the organ most affected; and which has greatest influence in spreading the disturbance through the general circulation. But it is important to the argument to observe, that there are numerous local influences in the vascular system, sudden translations of blood, and even well-marked changes in secretion, which cannot be thus explained; but depend immediately on the mental emotion forming the original cause of disturbance, and are only indirectly or slightly affected by the movements of the heart. It is needless to state examples of this curious fact, which is familiar in such various ways to common observation.

Besides the influence of mental emotions on the circulation, we have proof that it is often locally affected by the mere act of attention of mind, voluntarily directed to a part. Here again we have reason to suppose that the effect may occur without any intervention of the heart's action; but simply from the nervous influence, whatever it be, which associates each part of the body with the common sensorium;—that *internuncial function*—to use the phrase of a great physiologist—which establishes and maintains the unity of every living being.

The latter consideration brings us to the view of another relation between the nervous system (still using this term in its largest sense) and the circulation of the blood; illustrating some remarkable disturbances, real or apparent, in the latter function. This depends on the fact—much more strongly marked in many other animals than in man—that at different periods of life there is a varying degree of vital activity exercised by different organs; directly attested to observation by the quantity and manner of circulation of the blood sent to each. It is simply the expression of a fact in the animal economy, of which the examples are numerous and familiar. The various phenomena of puberty are perhaps the most striking instances in man. But one may be taken even from an earlier period of life, in that state of the brain during infancy and childhood, which seems connected with rapid development, and is indicated by great activity and mobility of the circulation through the organ. It cannot, indeed, be proved that any direct influence of the nervous system is the source of these actions; which, strictly speaking, belong to the same causes as original organization and growth, and lie under

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similar obscurity. But neither can we affirm that any altered state or distribution of blood is primarily concerned in them; and notwithstanding certain obvious analogies to vegetable life, it is safer perhaps to admit them in connection with the nervous functions, than with any other. Their effects, when interrupted or otherwise disordered, in producing disturbance throughout the whole circulation, are well known in the history of disease.

Regarding the manner in which the nervous system acts on the circulation, independently of its more obvious influence through the heart, the main question is, whether the general distribution of nerves through the minute vascular structure of each organ or tissue will account for this, of which local neuralgia may be taken as the simplest case;—or whether we must suppose also an especial power in the blood itself—what some have termed its innervation—to explain the vascular irritability common to the whole body, as well as that which exists, or is distributed, unequally to different parts. These are subjects where the intricacies of relation and invisible structure multiply around us; and where, in the same proportion, language tends to encroach on the reality of facts. Yet there are many experiments sufficiently distinct to show that nervous influence is expressly directed to the circulation in one or other of these ways, and possibly in both. I have already noticed the observations which indicate that injuries of the brain and spinal nerves are capable of variously affecting, or even wholly arresting, the movement of blood through the capillary vessels, independently of the action of the heart. And other experiments are related, proving that section of the pneumogastric nerve has remarkable effect on the blood itself, in diminishing the proportion of fibrine, and destroying the power of coagulation.

Though there is some ambiguity in these, and all similar results, yet are they sufficient, in conjunction with more familiar observations, to justify the view of a direct nervous action on every part of the vascular system and its contents. In the latter case, where this action is immediate on the blood, there may be reason to suppose that the whole circulating fluid is equally and similarly affected. On the other hand, the nervous influence which belongs to the vascular tissues seems liable to every variety of local direction or limitation. If this distinction were well established, it might explain, by the concurrence or separa-

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tion of the two influences, many of the complex phenomena referred to in this inquiry. But it must be admitted that we are far short of any such certainty; and that there is still a singular want of exact knowledge in all that concerns the relation between the nervous system and the circulation of the blood—the two greatest functions of the living economy.

We have evidence, indeed, that both the cerebral and ganglionic nerves are engaged in this relation; but in what precise connection with each other, or in what ratio to the several parts and functions of the vascular system, neither anatomy nor physiology have yet fully explained. The presumption undoubtedly is, from their obvious distribution on the trunks and branches of arteries, that the ganglionic nerves are those which chiefly give their appropriate irritability to the bloodvessels throughout the body. But here, as in all the relations of these nerves to other parts of the nervous system, we instantly lose every clue to further pursuit, and are obliged to limit ourselves to the expression of the simplest and most general facts.[1]

Pathology furnishes, as might be expected, many curious illustrations of this subject, though inadequate to the solution of the problems just stated. The most remarkable, undoubtedly, as being the most obvious in cause, are those derived from lesions of the brain and spinal nerves, and particularly from paralysis in its different forms. In the latter cases, we have often local changes in circulation, rendered very singular by the preciseness of their limitation to certain parts, in accordance with the distribution of particular nerves. This is well seen in certain slight and partial paralytic affections, where a portion of a limb, or even one or two fingers, may become almost bloodless, while the others retain their natural state.[2] I find among my notes many such instances of partial change in arterial circulation;—some in which the effect was manifest even in the larger trunks of arteries leading to a part under this influence. An example has recently occurred to me of slight hemiplegia, evidently connected with cerebral disease, where the beat of the arteries on one side

[1] The late Dr. Fletcher, in his remarkable work on the Rudiments of Physiology, gave a more extended form to this doctrine of the ganglionic system, describing it as the source of all irritability in the body.

[2] I have also seen cases where the perspiration of a palsied limb was singularly altered in the quality of the matter perspired.

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the head was wholly different in character from that on the other, as shown even in the carotid itself: and similar differences probably exist in many cases of this nature. The strong beatings, which sometimes occur in the course of particular arteries, are well known; and though we may hesitate to describe them, with Laennec, as neuralgic spasms of the artery, yet it is difficult to attribute them to any other than nervous influence of some kind on the coats of the vessels so affected.

It is a question whether we can assign to the same cause, that rarer occurrence, where every part of the arterial system, to which we have access, is in a state of irritable action at the same time; with a peculiar jar or vibrating beat, not unlike that attending some morbid states of the heart, yet here manifestly derived from another source, by which this organ also is simultaneously affected. There is a good deal of analogy between this condition, and what may be termed an hemorrhagic state of the arteries; and there are some actual varieties of hemorrhage admitting of illustration from this source. It may possibly be produced by particular changes in the blood itself, thus acting on all parts of the vascular system;—or from a state of the inner coats of the arteries, described by some authors, verging on inflammation. But I have seen one or two of these cases so singularly connected with cerebral and nervous irritation, as to make it probable at least that this formed the first step in the train of morbid actions.[1]

[1] In the most remarkable of these cases—a boy of four years old—the symptoms had existed ten days when I was called in, together with Sir Charles Clarke; during which time he had been bled to the extent of nearly 30 ounces. We found the child delirious; the pulse from 120 to 130; hard, and with a tense vibrating thrill in all the arteries, especially those of the head, which I do not remember equally to have noticed in any other case; and felt in the smallest artery to which touch could be applied. There was also a singular propensity to bite, apparently without passion, all those around him, even his mother and nurse;—and further, a constant dread of falling, in degree far beyond that which arises from mere debility, and evidently depending on the state of the brain. The delirium speedily ceased; but the fever, peculiar pulse, restlessness, and the other two symptoms just named, continued with little abatement for five or six days, and in slighter degree even for some weeks afterwards. At this time, nearly six weeks from the earliest date of the illness, dropsical symptoms came on, and rapidly increased; effusion taking place largely into the abdomen, and by anasarcous swellings all over the extremities; with evidence also of some effusion in the cavity of the chest. All the preceding symptoms—and notably those of cerebral irritation and disordered action

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I might take from another class of disorders—such as hysteria, in the largest medical understanding of the term—various illustrations of the influence of the nervous system in altering and disturbing the balance of circulation. But these are too familiar to require more than a reference to them, as part of the subject.

The preceding remarks, directed chiefly to those irregularities of circulation which are not inflammatory in the ordinary sense of the term, will show how wide is the topic, and how fruitful of questions still unresolved. Of the points severally discussed, the following may be stated as among the most important and best ascertained:—First, that the capillaries are, in numerous cases, perhaps in the majority, the first seats of these disordered actions, even where the causes reach them through the general circulation:—Secondly, that the direction of such actions, as well as their degree, has relation to the particular endowments, nervous and vascular, of each part, and to the functions they severally perform:—Thirdly, that the quality and quantity of the blood in circulation, but particularly the former, have also much effect in determining the nature and extent of these disturbances:—and, Fourthly, that there are further causes, producing or modifying them, in the influence of the sensorium and different parts of the nervous system; these causes operating, directly or indirectly, partially or generally, on the whole vascular system, exclusively even of the action of the heart.

It is unnecessary to point out further how integral a part these topics form of all pathology. I shall merely add a few remarks on the application of them (denoted in the title of this Chapter) to the Metastasis of Disease;—a copious subject, abounding in curious results, and still, it must be owned, inadequately pursued

of the arteries—abated in the same ratio as these dropsical symptoms came on. The latter were thoroughly relieved by calomel, digitalis, cascarilla, and chalybeates; and, at the expiration of about ten weeks, the child was restored to perfect health; without the occurrence of relapse, or the impairment of any organ or function of the body.

Some ambiguity may be supposed in this case from the effects of the large bleeding employed. But the narrative of the early symptoms gave reason to suppose that they had been essentially the same, before this depletion took place. Their sudden termination when dropsical effusions came on (here justly to be termed *arterial dropsy*), is a remarkable and illustrative part of the case.

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in its various details. The instances of the occurrence, fully ascertained and described, are probably few in comparison with those which actually exist under forms less explicit and obvious. The term of *Metastasis*, indeed, in its largest sense, might apply in some part to almost every form of disease. Scarcely is there one in which certain of the symptoms do not undergo change of place;—either from causes inherent in the nature of the malady, and therefore in regular series;—or from fortuitous circumstances in its progress; and that complex relation of parts and functions, which renders it scarcely possible that one organ should be morbidly affected, without disturbance to others also. The difference, however, as respects liability to translation, is great, even in diseases where only the latter causes are concerned. Some are prone to shift the seat of morbid action repeatedly, and on the slightest disturbance or provocation;—others seldom and tardily;—yet in each case, and in every particular disease, with a tendency to certain transferences, rather than others; obviously arising from some inherent difference of circumstances, and often marked enough to require description as a character of the disease.

A general view of the subject involves mainly the two great functions, which alone pervade every part of the animal economy; viz., the circulation of the blood; and the nervous system, in its several parts, connections, and sympathies. To these two influences solely, as far as we can see, must we attribute all translations of morbid action from one part to another. Most of the foregoing remarks on the causes which produce disturbance in the circulation, will be found to furnish this inference; and those not least, which relate to the variations in the quality of the blood, either from internal changes, or the introduction of foreign matters into it. For, in regarding the blood as an agent in the metastasis of disease, it is necessary to distinguish between the cases, where there is a mere transference of excess of quantity from one place to another, as in various instances of simple inflammation or congestion;—and those more remarkable cases, where a morbid state of the blood, or a morbid matter present in it, become the subject, and apparently the effective cause, of such translation. Of the latter, the transferences of action in the contagious exanthemata, and some other eruptive diseases, afford striking examples. And among metastases of this kind we may

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reckon those also, less regular in form, which occur in gout, and in scrofulous or scorbutic habits, forming a character in these several disorders, of great importance both in pathology and practice.

The two conditions then, just stated, regarding the blood, and the direct influence of the nervous system, may be said to include all the instances which can fitly be termed *translation of disease.* The division, however, so made, though convenient both in reasoning and practice, is by no means easy to follow in all cases; and we must admit it as certain, that the several causes run closely and imperceptibly into each other. It may be, for example, that the simple redundance of blood, transferred from one part to another, is an effect of nervous influence thus directed;—a question already considered; and showing, as do so many others, how difficult it is, in these ultimate actions, to discover the true relation of cause and effect. A similar doubt exists regarding the cause and manner of translation of morbid matters in the blood; and their relation to the inflammatory actions attending such change. These questions have also been discussed in some preceding chapters of this volume. They all include one consideration, never to be lost sight of; viz., that for every act of metastasis, however seemingly vague and irregular, there must be some definite cause; the discovery of which, whenever attainable, is equally important to the theory and treatment of disease. And this, it may be well to add, is a direction of research, where diligent observation can scarcely fail of success.

In every general view of the subject of metastasis, regard must be had to the influence of similar texture of parts, whether continuous or otherwise, in determining these changes in the seat of morbid action. Though this similarity is obviously not indispensable, since some of the most remarkable metastases are independent of it, it doubtless is concerned in many cases of much interest in the history of disease, and particularly in the affections of mucous and serous membranes. The disorders of the air-passages, the alimentary canal, and the skin, afford many illustrations to this effect; indicating at the same time the effect of continuity of surface and texture. And they illustrate, besides, in common with other organs, another very important fact respecting metastasis, viz., its frequent dependence on connection of function between the parts which are the subject of such

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changes. All these relations require to be kept sedulously in mind in reasoning on this matter.

The latter circumstance, viz., the effect of connection of function, might perhaps have been inferred, prior to all experience; but it is confirmed, as a fact in pathology, by instances exceedingly numerous, and often very remarkable. Besides those appertaining to the organs just named, we have many and striking examples in the urinary and generative systems, in the joints and muscular tissues, in the secreting organs, &c. And it may be to this principle that we shall eventually refer certain cases—as, for instance, the singular metastases occasionally occurring in cynanche parotidæa—which are wholly anomalous to our present knowledge, yet definite enough to prove a specific cause, the proper subject of future investigation.

The metastases—whether in the form of inflammation, congestion, or other morbid action—which are conjoined with, or depend upon, morbid states of the blood, are certainly amongst the most extraordinary phenomena of disease. Without repeating details, I may refer generally to what has been said in former chapters, regarding those translations in gout, scrofula, and other constitutional disorders, which, though they indicate simply various modes of development of the same cause of disease, come fairly under this denomination; and manifestly include many affections hitherto differently named and ascribed to different causes. For, however varying in aspect or importance, we are not entitled to separate in principle those metastases which occur suddenly, from such as occupy a longer time in the transference of disordered action to another part. In each case the blood must be the medium of translation, and the difference of time is one merely of degree. This manner of regarding the subject is essential to the inquiry; and it will be found to confirm the inference, drawn from other sources, that the effects of a simple morbid matter, present in the blood, may show themselves in forms greatly more varied than is yet presumed in the theory of disease.

Another point worthy of more notice than it receives, is the state of the body during what may be called the act of metastasis;—that is, while the morbid matter, wholly or partially dislodged from one seat of its action, is in passage through the circulation to another part. It is certain, on observation of such

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cases, that during this time of transference, whether longer or shorter in duration, there is often a very notable disturbance of the heart, of the nervous system, and of various organs seemingly out of the ordinary course of the disease; which disappear, when the symptoms are again locally fixed. I find in my notes many curious examples of this fact; drawn not only from the familiar occurrences of gout, but from numerous other disorders, —erysipelas, psoriasis, and other affections of the skin, certain strumous swellings, &c. The phenomena of the exanthematous fevers, and of all translations between the skin and internal membranes, afford instances to the same effect. The general fact might have been anticipated, seeing the certainty that every such transference of morbid matter (whether foreign to the blood, or an excess of some of its ingredients) must take place through the circulation, and that its influence cannot be wholly dormant during this time of passage. But I wish to draw attention to the circumstance, as one well meriting further observation, and leading to inferences beyond those which appear on first view of the subject.

Closely connected with the same general view of metastasis, are those cases, equally numerous as remarkable, where the translation forms what has been termed a critical termination of the malady; of which the abscesses occasionally occurring in fevers, and towards the close of many other disorders, are the most familiar example.[1] The change thus effected by what is often an inconsiderable deposit of purulent matter in some part of the body—remote, it may be, from those parts especially affected by the disease—is, in truth, one of the most singular facts in pathology. For there are cases in which we cannot reasonably interpret it as a mere transfer of irritation, but must look to the actual nature and amount of the discharge, as that which gives relief to the system; thus furnishing another argument for the doctrine which derives many of these idiopathic fevers, and analogous diseases, from actual changes within the blood itself. And the same principle applies, on the other side, to those various cases, where the sudden suspension of an habitual discharge brings on symptoms in other and remote parts of the

[1] The physicians of antiquity paid more attention to this subject of the crisis of disorders, whether by abscess or otherwise, than do those of our own time: and they are justified by its practical importance. I need not state proofs of what is so well known in their writings, from the time of Hippocrates downwards.

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body having no common aspect, yet manifestly to be referred to one specific morbid cause.

Other illustration of these views of metastasis may be drawn from the remarkable connection between certain morbid actions of the kidney, and disordered states of the brain;—traceable in every degree from the slight influences upon ordinary sleep, to the most serious affections of the latter organ; often occurring very suddenly; and manifestly depending, not on mere alteration of the quantity of urine, but chiefly perhaps on the imperfection of the secretion as respects the removal of particular ingredients of the blood, which are thereby injuriously retained in the system.[1] These cases have not yet been studied to their full extent, though of late much more carefully than heretofore. The same general remark will apply to certain affections of mucous membranes—bronchitis and asthma for example—which are frequently the subject of similar sudden translation;—involving in some cases, it may be, mere transference of blood, as where bronchitis supervenes upon hæmorrhoids; but in other instances, changes in its quality, produced by the new and altered secretions which take place.[2]

I have alluded already to the close connection by metastasis between the intestinal membranes and the skin;—one of the most important relations in the animal economy in state of health; and entering, more or less, into every part of the history of disease. The near equality in superficial extent of these inner and outer membranes, as ascertained by anatomists, might almost have suggested such relations, apart from the better evidence of facts. Disorders of digestion, alterations of secretion, changes of temperature, the influence of acrid or poisonous substances, and numerous other cases, are attended by a distinct reciprocity

[1] Some of the variations in the effect of wines and other fermented liquors, in producing intoxication, doubtless depend on this important relation between the functions of the kidneys and the brain.

[2] I have notes of some singular cases of this kind;—one, for example, in which there existed for many years a frequent and well-marked alternation of headaches with hæmorrhoids; each very severe in degree, and both almost wholly removed by the supervention of chronic bronchitis; which latter disorder has since continued, constantly and severely, for a long period of years. In the same case I have repeatedly seen a temporary translation for a few hours to the head, producing even delirium; which again was as suddenly relieved by the recurrence of a copious bronchial secretion. The connection distinctly marked between bronchitis and hæmorrhoids, I have noted in several instances.

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of action between the two surfaces; often marked enough to form a character for description; and in every instance deserving notice, both as pathological facts, and suggestions for practice. The rapidity of some of these translations, and the varying rate of others, is a remarkable part of their history; of which multiplied examples might be given, were they not rendered needless by their familiarity.

As the title of this chapter includes the subject of metastasis chiefly in connection with the circulation, I shall not do more than refer to that part of it which involves the more direct action of the nervous system in producing these changes. This is in itself a wide and curious topic of research; expounding the character of a large class of diseases, and embracing the whole subject of nervous sympathies, to whatsoever class of nerves they belong, and whether direct or reflex in kind. While thus slightly alluding to it, I may observe that a complete work on the Metastasis of Disease, in its most general acceptation, is still a desideratum in medical literature, and would possess great value. But to execute it rightly, all implicit adherence to names and systems must be laid aside. Some of the most important results are obtained from sources, and by observing relations, to which these methods of arrangement afford no clue; and which must escape notice, if the latter are rigorously pursued. Various examples to this effect have been given in the present and preceding chapters; and others will be furnished by the experience of every observant physician.

To the foregoing suggestion, and in connection with it, I would append another which at first view may seem incongruous, but which consideration will show to involve the same principle and points of research;—viz., the inquiry into *those morbid states or actions which are incompatible with each other, or cannot coexist in the body*. The importance and deep interest of this subject, as regards both physiology and pathology, need not be dwelt upon. If one morbid state or action cannot coexist with another, it is because there are positive physical conditions appertaining to each, of such nature that they cannot operate at the same time; certain of these states preventing the action of others, or interrupting and displacing them when previously present. The closer we press the argument, the more certain will it appear, that we have to deal with natural causes, however obscure their

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connection with other physical laws, or even with the particular parts and functions of animal life. No disease can be incompatible with another without a definite physical reason for its being so; and this, which is in effect a positive relation between the two, becomes a fit and most curious object of research.

The term of *incompatible*, however, can rarely be employed without qualification. The cases, though existing and familiarly known, are comparatively few, where one morbid cause in operation absolutely excludes the access of another;—much more numerous, where there is some mutual modification of effects from morbid causes coexisting in the system. The former instances chiefly occur among the contagious exanthemata; and the history of medicine abounds in curious examples of such interference;—sometimes by suspension of the effects of a virus received, in other cases by its entire exclusion. It may be that, even in some of these instances, the different morbid actions do really coexist and modify each other. For obvious reasons, it is far from easy to obtain perfect evidence, either here or in those other diseases, where we have knowledge that two or more causes of disorder are present in the system, exercising a mutual influence, and modifying all the symptoms thence derived. The examples most illustrative of this condition are those where casual maladies supervene upon constitutional diseases, gout, scrofula, &c. I have had occasion in several preceding chapters to refer to various instances of such interblending and mutual modification of morbid states; and the future progress of pathology is sure to enlarge and better define our knowledge on the subject.

In all these questions a basis of inquiry will be found in the relation of functions to one another in the healthy state. The very principle of incompatibility may be said to be hence derived; for here, as elsewhere, no interpretation of disease is so clear as that drawn from the natural conditions and changes of the body. However blended into a whole, each part and function of animal life has its especial limits of action; depending, obviously, on those mutual relations or relation to a common source of power, which makes a certain excess of action in one part a check or cause of cessation to actions elsewhere. Examples of this important principle are familiar in every portion of the animal economy. They are chiefly to be found in connection with the

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functions of the nervous system, and circulation of the blood—the two great actions or powers which alone pervade the whole frame. In the relation of these to the different organs of the body (a relation where it is difficult to consider them separately, seeing the endless ways in which their action is connected and concurrent) there is a certain balance or adjustment; which, though not to be defined by express terms of quantity, and ever varying in its proportions, yet has its limits, beyond which deviation cannot go without some notable infringement on health. This balance, broken or disturbed, may, in theory, be affirmed to affect in some degree every organ of the body. Practically speaking, the effects are only obvious and important where some particular organ or structure bears the weight of the change; and it is this class of cases which chiefly come within the scope of pathology.

It will be seen, even on the slightest consideration, how remarkably these various pathological views blend themselves together, and tend to concentration under laws more general than any yet recognized. It is under the sanction derived from this fact, that I have been led in several chapters of the present volume to bring various diseases together under new relations without regard to established systems of Nosology or nomenclature. Such new combinations are not only suggestive of common causes, and higher generalizations, but connect themselves more readily with those various discoveries by which modern science is extending and enriching every branch of physiology and medicine.

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CHAPTER VII.

ON THE INFLUENCE OF WEATHER IN RELATION TO DISEASE.

Amidst all the opinions and phrases current on this subject, it is singular how little real knowledge has been gained, or applied to practice. The difficulties of the inquiry are great, both from the complexity of the agents concerned, and from that of the organs and functions acted upon. Yet with the certainty we have of the extent, variety, and unceasing nature of this influence, it is manifestly a field of research, where patient observations and averages cannot fail of affording the most valuable results.[1]

On this topic, as on many others in this volume, the remarks which follow do not profess more than to give a certain degree of method to its several parts; and to suggest the conclusions, chiefly practical in kind, which appear to have engaged least attention. Some of the points, only touched upon, or briefly treated, might well furnish material for separate volumes, from their importance, and the basis they afford to experimental research.

The subject, taken in its whole extent, admits of being distributed into several separate objects of inquiry; the importance of giving a clear form to which may readily be understood. Complex in themselves, yet open in various ways to the common observation of mankind, it is especially needful to define as clearly as possible their nature, and the fittest methods of attaining them. Much knowledge is lost to the world by ignorance of these conditions. Facts and phenomena are seen, and perchance

[1] Attention to this subject is not always solicited by the sick themselves. Besides the accustomed looseness of inference on every point regarding health, it is singular how averse many patients are to attribute any effects to the influence in question. Another cause is invoked rather than this very powerful one.

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recorded; but they are lost again, because not brought into relation with one another, or associated under general laws.

The history of the diseases of different climates, though still deficient in many respects, and altogether wanting for many important localities of the globe, is perhaps that part which has hitherto been most successfully cultivated. For this object, however, observation is not to be limited to the effects of different meteorological conditions on the animal economy. It of necessity extends to many other local conditions, not fitly coming under this head; yet blending with and modifying all those which depend on climate alone, and requiring to be specified before inferences can rightly be drawn from the latter.[1]

The same may be said of that part of the subject which respects the climate of particular places, and their relative fitness as residence for invalids. Research here has been chiefly confined to the case of those laboring under pulmonary disease; and the observations with this view directed to averages of temperature, comparative quantity of rain, and prevalence of particular winds. Even, however, where it has been possible to attest these facts by averages of sufficient length, the difficulty remains of comparing other local circumstances; less obvious indeed, but which modify, and are often more than equivalent to, the direct effects of climate. And it must be added further, that this is a subject on which fashion, hasty observation, and mistaken methods of inference in the world at large, have very great influence. The loose and superficial notes of a few days or weeks are often made to interpret questions of climate, which many years of careful averages hardly suffice really to determine. Accordingly we find that our actual knowledge, though extended and better assured by two or three valuable works, is still wanting in certainty; and the practice founded upon it often singularly vague and of doubtful benefit.[2] The truth of the avowal will be ad-

[1] The want of exactitude in noting these local conditions, which I have often had occasion myself to observe, has been judiciously commented upon by Dr. James Johnson, in his work on the Diseases of Tropical Climates. Inferences, for example, can never be useful or admissible, which apply to the vast regions and various surface of India, as if it were one locality and a single climate. This is a strongly marked instance; but the comment applies to many others, where medical description partakes of that vagueness and want of attention to specialties, which belongs to all popular opinions on the subject.

[2] I may name as the best work we have on Climate that by Sir James Clark; very valuable in local details, and in the practical inferences founded upon them.

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mitted by every physician, who candidly reviews this part of the treatment of disease.

Another view of the subject, less exclusive than either of the preceding, is that of the relation of the several states and changes of atmosphere to the various functions of the body; a wide topic, abounding in curious results, which are of equal interest to physiology and pathology. Those which especially concern the former, though more or less at all times matter of popular observation, have only of late been received into the domain of science. The researches of Dr. W. F. Edwards and others, while adding greatly to our knowledge both in extent and exactness, show how much is yet to be learnt before we can appreciate the influence even of the most familiar agents around us. Every part of the history of disease affords the same inference. We feel these agents to be in perpetual operation;—some of their more obvious effects in producing disease are currently understood by all;—medical knowledge has registered a few others, in which certain disorders are manifestly produced, or rendered epidemic, by particular states of weather;—but there remains behind much more than has yet been acquired. We are still treading the shore, with a wide ocean before us for discovery. Scarcely is there a single branch of science which in its progress may not be brought to contribute to this object. And without claiming any present certainty for the connection which some physiologists affirm between electrical and nervous agency, it must be allowed that this part of science, in particular, is likely to explain hereafter some of the most singular, and seemingly anomalous, effects of the atmosphere on the human body.

The foregoing observations apply to the several modes under which this subject may be viewed; each having its peculiar objects, while all may be made to serve for mutual illustration. The remarks which follow have reference especially to the influence of certain states of air or weather in producing morbid conditions of the body;—either in their immediate effect;—or indirectly, by evolving or giving greater activity to other causes. The latter distinction must be kept in view, as far as our knowledge renders it possible; since, doubtless, many of the diseases of season and climate are derived from such secondary causes, rather than from those more obvious to common remark.

With this exception (under which we must include the exan-

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thematous and certain other fevers), the prevailing diseases of each season, as a class, are to be referred chiefly to the continuance, or more frequent repetition, of the causes by which particular cases of these diseases are produced. I need not, from their familiarity, enumerate those which are usually cited, though not perhaps with much precision, as severally predominant at different seasons of the year. Taking them collectively, they furnish certain average results of mortality, which form a very interesting and important branch of medical statistics, and have been greatly extended of late years in number and exactness; promising for the future various conclusions of much wider application than any we now possess.

On this subject more is often to be learnt by carefully and continuously noticing the phenomena of one locality, than by diffusing the research over many. Each mode of inquiry has its own value, separately and for mutual illustration; but that perhaps is most instructive, which connects the prevalence, type, and change of disease on a single spot, with corresponding conditions in the atmosphere around it. Observations of this kind, spread over a long period, disclose relations discoverable in no other way. They are important also, because more or less within the reach of every practitioner, and in their aggregate furnishing the material for those general conclusions, upon which alone reliance is to be placed.[1] In the first chapter of this volume I have spoken more expressly of the *method of averages*, as one of the valuable instruments and aids of modern science. Indispensable it may well be termed, in all that respects local climates, and their influence on human health; for trustworthy conclusions can come from no other source.[2]

[1] Some of the German reports, as those in the "Medicinische Zeitung" of Berlin, afford valuable examples of such research; the more so, from their associating the epidemic disorders of other animals with those of man. The latter is a source of knowledge and illustration not perhaps adequately regarded in our own country.

I must refer with the same commendation to the Statistical Reports of the Sickness and Mortality in the British Army, on different Colonial Stations, presented to Parliament, and published some years ago. Besides other important results, these Reports correct errors which have long existed as to some particular relations between climate and disease.

[2] With all their imperfections in physical knowledge, and deficiency of instruments for exact observation, the ancient physicians gave greater attention to the influence of weather on disease than has been commonly done at later periods. There is much that merits notice in their maxims as well as practical conclusions

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But besides the recognized diseases of certain seasons, there are others not in general so considered, which nevertheless are frequent enough, at particular times, to make it probable that atmospheric influence is concerned. My notes furnish me with many instances of this kind; some of them, indeed, liable to question, as being possibly the result of casual or unknown causes; yet deserving notice in the suggestions they afford for future observation. This part of the subject, indeed, connects itself with the larger question as to what have been called *epidemic constitutions of seasons and periods*;—a phrase denoting an undoubted fact, and one which has engaged the notice of physicians in all ages. It is among those many problems, equally important and curious, which are sure to be elucidated in the future, probably by methods and instruments unforeseen to our present knowledge.

It cannot be doubted that affections of the head, depending on disturbed balance of circulation through the brain, do at some periods much exceed the common average of these cases. The same remark I would extend, on my experience, to various neuralgic disorders; including under these some affections, usually termed rheumatic, which are thus, however, most correctly classed. Lumbago and sciatica are undoubtedly more prevalent in certain states of atmosphere: and we may recognize a similar effect in the familiar instance of pain renewed in old sprains, or in joints affected by former inflammation. Further, though more doubtfully, I have found reason to think that acute attacks of gout, in patients having this habit of body, are more common than usual at certain periods, without other cause to which this can reasonably be assigned. It has occurred to me to notice such frequency during the seasons when influenza pre-

on this subject; drawn from a climate more definite in its periodical changes than that of the northern parts of Europe, and furnishing, therefore, more ready and certain inferences. Some of these found in Hippocrates are admirably descriptive in themselves, and apply with singular exactness to facts every day present to our observation. I transcribe his account of the northern and southern winds, as an example of his vigor and accuracy of description.

Νοτοι βαρυηκοοι, αχλυωδεες, καρηβαρικοι, νωθροι, διαλυτικοι. Ην δε δυναστευη βορειον, βηχες, ψαρυγγες, κοιλιαι σκληροτεροι, δυσουριαι ψιρικωδεες, οδυναι πλευρεων, στηθεων.

Still better proof of his eminent faculty for such observations will be found in the remarkable books on Epidemic Diseases; the cases in which well deserve study, not on this point alone, but as models for every part of the history of disease.

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vailed; furnishing, if the observation be correct, a further example of the relations of this singular epidemic to other diseases.

With respect, again, to disorders of the internal membranes; while many of these are noted as prevailing at particular seasons, there are others not thus described, yet which observation shows to depend greatly on external causes, giving them frequently an epidemic character. Every physician must have noticed a class of bowel disorders, almost specific in kind, which prevail extensively at one time; while, at another, there is the tendency, not less marked, to different forms of cynanche, or to erythematous inflammation of the fauces, palate, and gums. Connected with these, I have observed in certain seasons, seemingly in damp weather long continued, a tendency to affections of the throat, passing down the œsophagus, and producing nausea, vomiting, or other disorder of the stomach. Erysipelas evidently prevails more generally and severely under particular states of weather, and these not connected with any regular occurrence or character of seasons. The infantile fever, however it be classed in our nosology, is clearly the subject of external influences, to the same extent, and possibly in the same way, as the exanthematous disorders. And with respect to gastric fevers in general, whatever name they assume, we have evidence of their becoming epidemic under certain continued states of weather; and these apparently alike at different periods of their occurrence.

The fact is a more remarkable one, that puerperal fever puts on an epidemic character at certain periods; this influence, whatever its nature, not being limited to one district, but diffusing itself over different countries simultaneously. Several years are recorded in which this disease spread itself largely over Great Britain and Ireland, France and Germany; and with very fatal results.

Hooping-cough may be further mentioned, as obviously partaking in the same liability. The different affections of the membrane of the air-passages,—catarrh, croup, asthma, and bronchitis,—though not all depending on this cause solely or equally, yet are greatly under its influence; and rendered more frequent or severe as certain conditions of atmosphere are present, or prolonged in duration. Even bronchocele, a disorder

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of which it is so difficult to render any plausible account, seems to afford another example of this influence. Other glandular affections are much more frequently and certainly submitted to it. One or two instances are known to me, where the ordinary prevalence of this complaint in a particular locality has been suddenly augmented by many new cases; sometimes numerously in the same family. The cynanche parotidæa is much more familiar as an epidemic at certain periods, in which effect weather may possibly be concerned; though it is more probable, from the specific and infectious nature of the complaint, that this is only partially the case.

In specifying these instances (to which many besides might be added), it is impossible to affirm that they are all attributable to those atmospheric states, which are familiarly understood under the name of weather. Other causes to which I have already alluded, may be the more direct agents on the body in many of the cases named; and in some are undoubtedly so. But as we know the production and diffusion of these agents to be greatly determined by variations of the atmosphere, and have little cognizance of other conditions affecting them, we may justly note and classify the various cases in which the state or changes of the air appear to act, as producing these disorders in the human frame.

Setting aside then what directly relates to incidental miasmata in the atmosphere (whether gaseous admixtures, animal or vegetable products, or other matters still less within our knowledge), any inquiry respecting the influence of weather in the production or modification of disease, must include four principal objects:—*first*, the temperature of the air;—*secondly*, its hygrometrical state;—*thirdly*, its weight;—and *fourthly*, its electrical state and changes. The agency of winds may obviously be referred in great part to one or other of these conditions. That of Solar light, though the facts are sufficient fully to prove its existence, is too subtle to be well defined in the present state of our knowledge. Nevertheless, seeing what has been done by science in expounding the physical conditions through which this great agent operates, and its effects on other forms of living organization, particularly on the growth and economy of plants, there is cause to believe that some of its relations to the body may hereafter be brought more expressly within our reach; and that they

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will furnish explanation of certain of the general phenomena and modifications of life which are yet obscure to our knowledge.[1]

While stating separately these conditions, it is manifest that none of them can really act singly upon the body; and this complexity of effect is a main difficulty of the research. Excess or deficiency in relation to a medium state, or the mere fact of change in any one of them, must presumably have influence, direct or indirect, greater or less in degree, on all the rest. Meteorology (itself only beginning to take place among the exact sciences) has hitherto done little to meet the perplexity of these questions. Evidence of what is yet wanting to it may be drawn from the extent and variety of research now directed to remedy these deficiencies. The direction, velocity, and physical causes of wind and hurricanes;—the phenomena of evaporation and dew, and the nature and manner of formation of clouds;—the causes of the deposition of water as rain, hail, and snow; and the relative quantity of rain in different localities and at different heights;—the averages of heat under every variety of climate

[1] Dr. Reid, of Edinburgh, has inferred from experiment, that the effects produced by an atmosphere loaded with excess of carbonic acid, are more speedily removed when the patient is placed under a strong light, than when merely brought into fresh air. In conformity with this, it has been found that there is greater proneness to disease in those parts of crowded buildings, such as barracks, from which light is most excluded; a fact of which Sir James Wylie's experience in Russia affords some striking examples.

When the first edition of this volume was in the press, the discoveries of Daguerre and Talbot had just been announced. In the short interval since, photography has become an important branch of experimental knowledge, as well as a refined and beautiful art. The enlargement it gives to our views of the chemical agency of light, is the great feature of this discovery: but extended as the research has since been, it may be said to unfold other new conditions of this great agent (*lo ministro maggior della natura*), both in regard to its own constitution, and to the ponderable forms of matter on which it acts. The term *unfold* has more peculiar fitness in this case, where modern science has disclosed in the solar spectrum not only three primary rays of color, wrapt within each other in different proportions, but also calorific rays, and others of chemical action, similarly diffused, in varying ratio, over the whole spectrum, and even beyond its visible boundaries;—and yet further, an interlacement of dark lines (some hundreds of which have been distinctly noted), fixed, invariable, and originating apparently in the source of light itself. This complexity of parts in the single beam becomes yet more wonderful when looking to the character of those movements in space and time, which may be said to form the essence of light, as far as it is cognizable by us;—physical conditions which man has found means to express by numbers, but which the hardiest conception is unable to follow in thought.

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and locality; its distribution by isothermal lines, its degree at different elevations, and under the various influence of reflection and radiation; and the temperature of space; the barometrical states of the air; and their irregular or periodical changes, as produced by heat, electricity, the lunar attraction, or the ocean tides;—and the electrical or magnetic conditions of the atmosphere, in their wide and various relations;—all these phenomena, as well as their mutual actions and dependencies, are now the subjects of active and successful inquiry.[1]

The difficulties arising from these complex conditions are greatly enhanced by the variety of the living tissues upon which these agents have severally or conjointly effect; and, it may be, by other relations of their action to the laws of life, of which our knowledge is limited at present to a few general intimations.

For, in considering this subject, a foremost point for notice is the susceptibility of other animals to atmospheric changes, which we are altogether unable to appreciate; a phenomenon often so remarkable as almost to warrant the notion of other senses than those possessed by man. Instances of this kind are too familiar

[1] We have remarkable proof in several recent discoveries respecting winds, temperature, and rain, of the ignorance or error before prevailing as to some of the most familiar of these facts; and even the most striking of the discoveries thus made, give larger view of what remains behind for future attainment. The theory of hurricanes, so successfully developed of late, and rendered capable of such important practical uses, leaves it yet uncertain whence are derived these vast and violent whirlwinds—what is their connection with other winds—what their relation, of cause or effect, to the remarkable electrical appearances which usually attend them—and how they produce such sudden and extreme barometric changes. There are few topics in this branch of science which do not afford illustration of similar kind.

It is a part of the sagacity of true genius to interrogate nature in her most familiar workings, seeking for causes and relations among phenomena, which others disregard from their seeming simplicity. The Memoir on Thunder and Lightning, by M. Arago, composed for the *Annuaire du Bureau des Longitudes*, 1838, is an eminent example of this method of philosophy, and singularly attesting at the same time the truth of what has just been stated. Of the variety of phenomena comprised under this title, whether purely physical, or such as affect organized life, there is scarcely one which can be regarded as altogether solved; and the greater number have barely been touched upon by speculation, without actual experiment or proof.

I would further refer here to an interesting Memoir on Atmospherical Phenomena by Professor Kaemtz of Halle (*Schumacher's Jahrbuch*, 1838), and to the great work of Professor Dove of Copenhagen (which received the Royal Medal from the Royal Society in 1853), as giving evidence of what is yet required on this subject, and of the proper methods of observation by which to attain it.

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to need recital. They are furnished numerously from every part of the animal kingdom; and many doubtless exist, especially in insect life, more singular than any of which we have knowledge. Strictly considered, however, they assure us of nothing more than a higher power and keener susceptibility of the ordinary organs of sense. Without denying the possibility of others, we cannot regard the exquisite perfection in which many animals possess from nature the senses of smell, sight, hearing, and touch—or even the extent and exactness which these attain in man from constant and earnest exercise—without admitting the likelihood that new phenomena both of sensation and action may arise from this greater acuteness. It is in some sort an ingress to a new sphere of life, where particular avenues are thus enlarged; and perceptions admitted inaccessible to organs otherwise constructed, or less exercised.

Some of the phenomena of animal instincts clearly lie in this direction. The conditions and changes of weather are so urgently important to a vast number of species—connected as we find them to be with the great instincts of food, shelter, and propagation—that provision was to be expected to meet these necessities; and such is actually found in an organization capable of being affected by changes, of which man is wholly unconscious. Acts, which seem to us altogether to anticipate the need, are in them the result of sensations already received, and leading to this result.

In considering the separate qualities of atmosphere already mentioned, in relation to their morbid effects on the human body, the same remark applies, as in so many other parts of the history of disease; viz., that the agencies are all natural, or even salutary, in degree; and that it is only the excess of state, or of change, which becomes a source of disorder. And further, that the susceptibility to be acted upon by each varies greatly in different individuals;—conforming in this to the diversity in degree of all other impressions, as determined for every one by his respective idiosyncrasy. We have much here still to learn; and it is a subject where there is reasonable prospect of future success.

Taking these three several qualities of atmosphere in the order I have proposed for their examination, that of *temperature*

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comes first; the most familiar of these conditions, and that which has been chiefly studied, both in its direct effects on the body, and indirectly through the diseases of climate. As respects the former, it seems certain that changes, sudden or frequent, are principally concerned in these results.[1] The power of accommodation in the body, depending on the generation of animal heat, and on the functions of the lungs and of the skin, provides in the healthy state against all which are not in excess. But where these functions are impaired, or the body otherwise disordered, every such change has influence; either by disturbing the balance of circulation between the external surface, and the membranes or different glandular structures within the body;—or by checking or augmenting the discharge of perspirable matter;—or in part, it may be, by more immediate action on the nervous system; though of this we have less certain proof.

The degree in which external cold may alter the balance of circulation—directly, by contracting the capillaries and smaller arteries of the surface; or indirectly, by the effect of this altered balance upon the action of the heart itself—is scarcely enough regarded in its various details.[2] It is to be presumed, on the most common grounds of estimation, that the differences thus made may vary (according to the degree of cold and the powers of reaction from within) from the smallest assignable amount, to that of several pounds of blood, changed in its manner of distribution through the vessels of the body. The importance of such fluctuations must be obvious on the most general view. And they include, it may be added, not merely the repulsion of blood from the surface by the contraction of the capillaries, but also the effects of the reaction and return of blood to the part; the latter consequences often very remarkable in their influence on the bodily functions.

The tendency of sudden changes of temperature to produce topical inflammation, is doubtless owing chiefly to these disturbances in the balance of circulation, which arise from changes,

[1] *Αἱ μεταβολαι μαλιστα τιχτουσι νουσημματα, χαι αἱ μεγισται μαλιστα.—Hippocrates.*

[2] The experiments of M. Poiseuille, in his treatise on capillary circulation, confirmed by those of M. Magendie, show the effect of a low temperature in retarding or preventing the passage of blood through these extreme vessels.

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general or partial, in the capillaries of the surface. Rheumatic affections, whether inflammatory or not, are usually attributed to the same cause—rightly as respects some states which bear the name; not so, as to others, which are undoubtedly of different origin, and affect different parts of the animal texture. Many disorders of the serous and mucous membranes, of the lungs, of the alimentary canal, and other viscera, depend more certainly on changes in the distribution of blood thus made; either suddenly, or by continuance and repetition. And these also are among the changes which have direct influence on the brain; the result of various averages, showing that apoplectic seizures are most frequent when either heat or cold are severe in degree—the mode of action doubtless different in the two cases; yet in each depending chiefly on disturbances created in the movements of the blood.[1]

The influence of external temperature on the functions of the skin, whether those of transudation or simple evaporation, is scarcely yet fully estimated, though the researches of Dr. Edwards and others have done much to extend our knowledge on the subject. The changes so made, either in augmentation or diminution of the natural discharge, are obvious and often very great. Without reciting the observations directed especially to these points, it may be remarked that a natural provision against injury exists here, as in the case of the temperature of the body, in the diminution of other excretions, and in the relation of absorption to the matters perspired—a remedy inadequate, indeed, to repair extreme or continued losses, but sufficient for all the ordinary occasions of life. This subject belongs, subordinately with that of heat, to the general doctrine of climates; the influence of which on the animal economy is regulated, in part, by the provisions just named; in part, by actual changes in the state and texture of the integuments of the body; exclusively of those further modifications which depend on the usages of life in each country or community.

The effects of perspiration suddenly checked by external cold

[1] Regarding the coup de soleil, usually cited as one of the most striking examples of the effects of heat on the brain, we have some recent evidence (though hardly decisive) to show that the change thus suddenly induced belongs rather to the pulmonary circulation, than directly to the head. This is contained in a paper by Mr. Russell, surgeon of 68th regiment, at Madras; read before the College of Physicians some years ago.

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are the subject of general apprehension, and influence many of the details of medical practice. Though in some instances mischief may arise from this source, I believe the alarm to be unwarranted in degree; and many of the effects, so attributed, to be due to other causes acting concurrently; such as exhaustion from fatigue, the perspired fluid left on the body, and the influence of cold itself in suddenly changing the balance of circulation between external and internal parts. The latter effect may equally happen, independently of perspiration; and there is no ascertained reason why this, previously occurring, should alter or change its amount. The customs of some countries, and the necessary habits of particular avocations, show how suddenly these changes may be made without any injury, if other causes of mischief are excluded; and prove the uselessness or wrong selection of many of the cautions current on the subject. This is a point on which just views are very desirable to the practitioner. It is in every case important that his judgment should be unfettered by common opinions, exaggerated or unproved; and though here, as in other instances, it may be well to concede sometimes, yet must he ever maintain the prerogative of applying his better knowledge, when circumstances require it.

It is not easy to say exactly how far practice might prudently run in an opposite course. As respects climate, indeed, the commingling of conditions is such, that to prescribe cold climate as a remedy would not only seem a bold anomaly in practice, but an object scarcely to be attained, without incurring other effects, wholly apart from temperature. The recommendation of warm climate has a specious sound, and is in reality perhaps more consonant with the habits and necessities of English life. Nevertheless, though this change be better suited to the greater number of cases that come before us, yet is the recommendation of a colder climate not wholly without sanction in some disorders, and still more as a means of preserving health in particular habits. Admitting freedom from organic disease or any active disorder, two or three winters of Canadian life will often do more to invigorate a languid habit, than the same seasons passed on the warm shores of Nice, Naples, or Palermo.

To the more common results of variations of temperature, hitherto noticed, may be added those which depend on extremes of heat or cold, suddenly or continuously applied to the body;

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the observations regarding which have been much extended of late years. The recent voyages of northern discovery furnish many as to the effect of high degrees of cold, of great interest to physiology; but as these are now familiar, and do not apply to practice, I merely allude to them as one portion of the inquiry.

There are other parts of this subject, more practical in kind, which, though better considered now than formerly, do yet not receive all the notice they deserve. Such are the direct applications of cold as a remedy; possessing certainly great value, and admitting of much more general and defined use than is made of them. Common prejudices, fostered to a singular degree on this point, are not only a great hinderance to the physician, but often do much to pervert his own views and practice. The old phrase of "*catching cold*," applied to catarrhal affections, is as deeply embedded in English opinion, as it is in the English language; and may well be considered a fertile source of error and mischief. Accordingly we find that the effect produced for a time by the writings of Dr. Currie, on the application of cold in fevers (exanthematous as well as others), has been only partially sustained; and that the common course of treatment scarcely goes beyond the removal of the old and noxious errors of close atmosphere, hot rooms, and thick clothing;—doubtless a very beneficial change, but not precluding the more direct and extensive application of cold to the surfaces of the body. I say *surfaces*, because in fact cold acts remedially on the lining of the alimentary canal, as well as on the outer skin; is often as imperatively required by the sensations of the patient; and not less sanctioned by the good obtained. Iced water has better title to a place among remedies in febrile and inflammatory disorders, than many of more ambiguous use, which have been transmitted from one pharmacopœia to another.

Whatever the theory of this action on the surface of the body, the benefits gained are incontestable;—familiar to all who have fairly employed it, and well recognized by patients themselves. Almost may it be taken as a rule, that wherever there is a hot and dry skin, cold in one degree or other may safely and expediently be applied to change its state. The benefit of simple abstraction of heat is great in such cases; and the fact is not sufficiently adverted to, which I have often put to thermometrical test, of the extent to which this influence is diffused beyond the

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surface to which the cold is immediately applied. There is no real risk here to countervail the good gained. We are sedulous in providing for and varying the application of heat to the body; while, from one cause of alarm or another, little provision is made for the opposite remedy, though not less capable of being actively and beneficially employed.

A point subordinate to this, which has had less notice than its practical importance deserves, is the influence of cold or hot air, respectively, upon wounds or open surfaces. The greater sensibility of parts so exposed, and the more direct actions on their vascular texture, make this condition a very important one. And accordingly we have much proof in private practice—still more from the experience of hospitals and military campaigns—of the effects produced by heat and cold severally, or by changes from one to the other. I have seen this remarkably in the army hospitals in Portugal; where, in summer, the general rate of recovery from wounds was accelerated or retarded, as the temperature became suddenly cooler, or the reverse. It is singularly attested in the instance of the wounded, left exposed on the field of battle.[1] Though the better understanding of ventilation has contributed towards this object, yet might much more benefit be derived from the direct effects of cold as an antiphlogistic means; either through the atmosphere, or by immediate application to parts affected. The employment of cold water externally, as a dressing to fractured limbs, gives one proof among many of the benefits of the latter practice; and we have reason to believe that this liquid, in its simple state, forms one of our best applications for the relief of inflammation in open wounds, or other inflamed surfaces.[2] Here also prejudices are to be overcome; the best assistance towards which is often that derived from the sensations of the patient himself.

While thus briefly referring to some of the effects of temperature, and chiefly on points of practical import, it must be re-

[1] In Lord Clarendon's narrative of the battle of Edgehill, there is a curious evidence as to this fact in the case of Sir Gervase Scrope, and another officer, whose recovery, after many and severe wounds, was attributed by the surgeons to their having lain stript on the field of battle for forty-eight hours in very cold weather.

[2] Dr. Macartney, of Dublin, in his Treatise on Inflammation, has largely and ably illustrated this practice, both as matter of history and present experiment; uniting it with his own doctrine as to the general nature of inflammation.

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peated, that we can rarely view them separately from the other conditions before noticed. Every change as to heat or cold in the atmosphere must either be the effect of, or produce other changes of atmospheric state; and none of these, it may be affirmed, are wholly indifferent to the body. Even in the simple case just mentioned, of the influence of warm or cold weather on open sores, though the atmosphere be admitted as the source of change, the effects are probably not due to temperature alone. Still less can it be supposed in regard to certain winds of our own climate; such as those from the east and south; the relations of which to the body are in no wise interpreted by the thermometer. The same observation extends more remarkably to the Sirocco of the South of Europe; and generally, perhaps, to the dominant or more peculiar winds of every locality over the globe. Where any one is especially noxious in producing epidemics, or in its effects on the general health, there, probably, is the direct influence of temperature on the body least in proportion to the other causes concerned.

Even in the endemic diseases of particular climates, the same view may be entertained. We have no certain proof that the fevers of the West Indies, or the Guinea coast—or the dysentery, remittent fevers, and liver diseases of different parts of India —or the malaria-fevers of Italy and Greece, are owing to the heat merely of these several climates. Hepatic disorders generally, indeed, may be considered as having closest connection with this influence; but in others of the above examples, the best evidence we possess leads us to causes in which temperature is only indirectly concerned.[1] And though this evidence be notoriously imperfect, yet is it valuable in the direction thus given to further inquiry. We have no direct cognizance of those miasmata, whether of animal or vegetable origin, or simply chemical in kind, which form the material of epidemic disease;

[1] Here I may again refer to Dr. J. Johnson's book on the Diseases of Tropical Climates, in which he shows how vaguely these relations of disease and locality are often considered, and made the subject of inference. We speak of hepatitis and remittent fevers as diseases of India, without adverting to the fact that the true hepatitis (or that which is not a sequel to fever) is ten times more prevalent on the coast of Coromandel, than in the plains of Bengal;—intermittent and remittent fevers in an equal ratio more frequent in the latter locality. The medium annual temperature of Madras is known to be amongst the highest on the globe (88° Fahr.); that of Calcutta about ten degrees lower.

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but we know that such material emanations exist; that they differ in different localities; and that variation of temperature is the condition seemingly most essential to their several forms and various activity. We have evidence, both experimental and of natural occurrence, of the effects of a certain degree of heat in producing or evolving these agents; and of a higher degree in destroying them, or suspending their action. Such results might be inferred as probable, from what we have cause to presume of their nature; looking here, as the nearest analogy, to the chemical constitution of the known poisons, whether of animal or vegetable origin—to the feeble affinity by which their elements are generally united—and the facility with which they are decomposed, and enter into new combinations from slight changes of temperature alone.

I need not refer to the many illustrations of this subject furnished by the history of disease. They are continually multiplied, as observation becomes more exact; and it is likely that the estimate of effect from this source will enlarge in proportion to our knowledge. The unequal influence of equal averages of heat in different localities might itself suggest doubts whether too much is not attributed to its direct action, too little to its operation through other agents. All examination of particular local conditions, such as soil, elevation, general humidity, quantity and kind of vegetable growth, manner of culture, and extent of running or stagnant water, shows the singular importance of these circumstances, as determining the endemic disorders and average health of different localities, exclusively even of the habits and employment of the people in each. Every country and district furnishes such instances; and all concur in proving that we must estimate the influence of temperature upon the body, and especially of heat, subordinately in great part to these more varied conditions. Isothermal zones would afford a very uncertain measure of the character or prevalence of disease.[1]

[1] Many excellent papers on this subject have appeared of late in the Transactions of our provincial Medical Associations based on that statistical method, which alone is capable of affording sound results. They all show the intimate relation between the nature of the surface and the prevalence or infrequency of particular disease in given localities; a point in which external temperature is only indirectly concerned, but where the effects are of singular importance in a practical view. Long and careful averages can alone be effectual in expounding them, by removing gradually all extrinsic or accidental circumstances.

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But it is a further question here, whether variations of atmospheric temperature may not induce a state of body, rendering it more liable to receive specific infections, though these be generated by agents without? That there are such differences of bodily condition, however vaguely known to us by external signs, must be admitted. And it is perhaps not a rash inference, from the temporary effect of exposure to great heat in quickening the circulation and augmenting the animal temperature, that continued exposure to the same cause, even much less in degree, may keep the constitution in a state prone to morbid actions, when the exciting causes are present. The uncertainty in this case depends in part on our ignorance of the equality of the causes, and of the relative degree of exposure to them; and can only be met by strong presumption, or actual observation of change in the bodily state. But I think it improbable, seeing especially the small increase of animal temperature from elevation of that without, that heat alone is concerned in producing such alterations: and, if depending on atmospheric causes, it is likely that these are of mixed kind, and blending with other actions more peculiar to the body itself.[1]

This may be the place to notice, however briefly, a topic which has ever been of much interest in English practice; the influence, to wit, of particular temperature on the origin and different stages of pulmonary consumption. Physicians have now fully recognized the error of supposing that a disease, thus specific

I may again notice here the Statistical Reports lately published of the sickness and mortality in the British army in different colonial stations; and the first Annual Report of the Registrar General; both documents most valuable in kind, and the latter promising, in its extension, results which will bear upon every branch of medical knowledge. Among other recent works on the subject of this chapter, I may mention Quetelet's Memoir on the Influence of the Seasons on Mortality in Belgium; and Dr. Fuster's work on the Diseases of France in relation to the seasons.

[1] The best observations we possess, show that the change of temperature in the human body, made by extremes of natural climate, does not exceed one or two degrees. The experiments of Berger and Delaroche, on the effects of exposure to higher and more sudden heat, prove that a temperature of 80° Fahr. above that of the body may raise the animal heat eight or ten degrees; a grade still below that evolved in some fevers, and under particular lesions of the nervous system. It is important to notice, that the same conditions produced different results to the two experimentalists; an effect that might have been anticipated, seeing its probable dependence in part on the excitement to circulation, which is so various in different individuals from the same causes.

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and constitutional in kind, can have its origin in cold merely, or any single condition of temperature. All recent evidence would even tend to show that of the two characters of climate, hot or cold, the former is more prejudicial in evoking this malady.[1] But allowing for some ambiguity in these results, the inference at least must be, that its origin is distinct from any mere locality; and that the influence of climate, as respects temperature especially, is that chiefly of retarding or otherwise modifying its appearance and course, where the tubercular diathesis is not very strong in the habit. These, indeed, are important benefits, and sedulously to be sought for in many cases. But much, it must be owned, is yet wanting to give consistency and usefulness to this part of our practice, as regards both the selection of localities, and the adaptation of them to particular stages of pulmonary disease, and to the combination of these with other disorders, or liabilities to such, in the general habit.

This, however, is too wide a subject to enter upon at present, where the more direct influence of heat or cold is the point with which we are chiefly concerned. I will merely state the probability, as a general fact, that changes of temperature, frequent or sudden, and the conjunction of a moist atmosphere with such variations, would seem to be the conditions of weather or climate, on the whole, most injurious to the phthisical constitution of body, in aggravating, if not in evolving, the active symptoms of the disorder. The specific cause still remains behind; hereditary in its nature; apparently affecting some races or communities much more than others; and only partially subordinate to the influences which climate or manner of life throw around it.

The action of cold, regarded under the general view of locality or season, is perhaps less remarkable than that of heat, as not equally involving those physical agents which become the direct causes of disease. But besides its effects on the balance of circulation already noticed (and which, though more strikingly shown by sudden changes of temperature, are also a result of continued cold), we have to notice its indirect influence in producing certain habits and necessities of life which variously affect the health; and more especially the alteration it makes in all that relates to food in those countries, where it gives the predominant character to the climate.

[1] See the Army Medical Reports on the Malta and Jamaica stations. On this subject at large the two works of Sir J. Clark may advantageously be consulted.

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The same manner of reasoning on the morbid effects of heat and cold, whether immediate, or such as depend on long exposure, must lead us to make large allowance for the momentary condition of the body, and the general habits of life. A man under strong exercise, or with habits of such, is very differently affected from one in repose. Protection from, or exposure, to the causes which augment the direct influence of temperature, as the open sun, wind, and rains—comfort or privation in the manner of life—habits of temperance or sensual excess—even the different occupations and temper of mind;—all these conditions modify more or less the effects of heat and cold on the body; and some of them, in particular cases, so powerfully, as almost to invert the accustomed results of such exposure.

In practice also, and for a rule in the habits of life, regard is not sufficiently paid to the different power which different individuals possess, of generating animal heat. This function, whether depending on changes in the blood and manner of circulation, or more directly on the nervous system, is as various in its power and exercise as any other of the body, and requires to be dealt with as such. Each age, too, has its changes in this respect, as well as every condition of health; and precautions founded on them cannot expediently be neglected, provided they are not so minute as to interfere with other parts of the economy of life, equally essential to the welfare of the whole.

In a brief outline like this it is needless to particularize instances. They are familiar to common remark; cited in medical works (though not always so specifically as the subject requires); and are very striking in the more extreme cases, where the struggle between the agency from without, and the powers of resistance from within, is most strongly marked. For we must ever revert to those great provisions in the constitution against all extreme or sudden changes of external temperature, by the laws which govern the production of animal heat, the action of the exhalants of the lungs and skin, and possibly also the secretions of other organs.[1] No correct results can be obtained as to the agency of heat and cold upon the body, without keeping these powers of balance constantly in view;

[1] Taking the record, seemingly well authenticated, of the two extremes of temperature of the human body, as determined by diseases affecting the blood, we find them to include a range of nearly 40° of Fahrenheit.

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and as they again are perpetually undergoing modifications from the various conditions of life, so is there a circle of relations, tending altogether to equality of average, though greatly broken and interrupted in its several parts.

We have no evidence of equal provision, as respects the second of the general conditions of the atmosphere, viz., its *hygrometrical state.*[1] But, on the other hand, there is every reason to infer that no similar need exists for it. The simple agency upon the body of dry or humid air, is doubtless much more limited in every sense than that of heat and cold—restricted as far as we can see, to certain organs, and less powerful in its influence on these. It is still more difficult also to detach it in observation from the influence of other causes. Sudden and considerable changes do not occur in the hygrometrical state of the air, without corresponding changes in its temperature, weight, and electrical condition; and it is probable that the two latter conditions especially have much more concern in the influence of damp weather on the body, than the mere proportion of moisture in the air. Even the common fog, or mist, is far from being a single or simple phenomenon. In some instances it is the cloud already formed, and brought by currents of air or other causes to a lower level—in other instances, as in the fogs which occasionally intervene between thunder-storms, the result apparently of a change going on in the electrical relations of the earth and atmosphere at the spot, producing alterations in the hygrometrical state of the latter. Science has not yet assigned their proper place to these several changes, as regards the relation of cause and effect. But however this be determined hereafter, the complex nature of their action on the body still remains, and will long retard any certain conclusions on the subject.

Another source of ambiguity, in considering the effects of different degrees of humidity of the air, is the influence of local circumstances of soil and surface in modifying this state; especially in that lower stratum of the atmosphere, with which man has chief concern. Such modification regards not merely the

[1] Unless, indeed, we admit as partially and indirectly such, the apparent relation between perspiration and absorption; the latter process balancing, by its increase or diminution, any changes the former may undergo from the different conditions of the atmosphere as to moisture or dryness.

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quantity of water taken up by the air, or precipitated from it, according to the several conditions of the surface, and the action of external sources of temperature; but also the various miasmata disengaged, or otherwise acted upon, by the same processes. I have already adverted to these material causes of disease, in their more particular relation to heat. Whatever their nature (and we have everything still to learn here), it seems certain that the presence of moisture, either upon the surface or near it—under the form of vegetation, damp air, or soil, and acted upon by a particular degree of temperature,—contributes much to their production, if not indeed essential to it. And to these conditions, conjoined, perhaps, with the electrical state of the atmosphere, we may chiefly attribute the greater unhealthiness of the rainy seasons in tropical climates, which the mere quantity of rain falling will not sufficiently explain.

But further than this, there is some cause to presume that aqueous vapor in the atmosphere, whatever its mode of combination, is much concerned in giving activity and spread to these miasmata as the cause of disease. It is idle to speculate upon physical relation here (whether that of solution, or of independent elasticity, according to Dalton's theory of vapors), while so entirely ignorant of the chemical constitution of these agents. We can only affirm that the conditions which concur to their production, are likely to aid in their diffusion and action on other bodies; and though the proofs are by no means assured, yet there is evidence that a foggy and humid state of atmosphere is that in which contagious or epidemic diseases are most readily and extensively spread. Other causes, however, doubtless operate, and produce many apparent or real exceptions to the fact.[1]

Recurring to the more direct influence of air, loaded with moisture, on the body, we have reason to expect it to be greatest on the functions of respiration and of the skin; and observation, as far as it goes, confirms this view. The effects in each case are probably owing chiefly to the altered amount of discharge from the exhalant vessels of the organs concerned:—in part also, especially under a low external temperature, to the

[1] Dr. Macculloch's views, in his work on Malaria, are carried beyond what facts will justify; but they bear the impress of that acute observation which distinguishes all his writings.

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greater effect of cold, when conjoined with moisture, on the capillary vessels and sentient extremities of nerves of the surfaces exposed. The influence of the latter cause upon the whole animal economy has perhaps not been enough noticed. The difference to the feelings between a temperature of 45° Fahr., in dry or in damp air, is one however which cannot escape the most ordinary attention. It is a difference equally marked as that between steam, and air heated to 212°, in their respective application to the body. The membrane lining the air-passages is obviously most liable to these effects, and to disorders depending upon them; as the experience of patients suffering under asthma and bronchitis, however varying in details, painfully testifies in its general results.

For the reasons already given, we are rarely indeed entitled to speak of humidity alone as a morbid cause; but it undoubtedly concurs with and renders others more effectual. And in the case of very damp air received into the lungs, it is probable that it may act expressly by retarding or impairing the changes made in respiration; and especially those depending on exhalation, which form so important a part of this process. Modern research, in showing the facility with which these changes take place (not merely by vascular structure, but through intervening membranes, and dependence on more general physical laws), exposes in the same ratio their liability to be altered or impeded by causes which before scarcely came into our view.

The action of very dry air on the body is even less certainly known to us. There is reason to believe that the effects of the Simoom wind (exaggerated, perhaps, in common narrative) are due in part to this cause;—in conjunction with its singular heat; the quantity of minute sand, or ferruginous matter, it conveys; and above all, the electrical condition of the current of air. There are more familiar reasons, however, for presuming that the atmosphere may occasionally be too dry (becoming so either naturally or by artificial means) for the healthy state of the functions of the skin and respiration. Without referring to the question, still undecided, whether absorption of atmospheric moisture through the surfaces of the body does occasionally or habitually take place as a natural process—and without affirming that the effect is derived from pulmonary evaporation unduly increased—we have various proofs that a state of air is often

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created by artificial heat, insalutary to the body; and that this condition may be removed by means which restore to it a certain degree of humidity. Houses or apartments heated by stoves (particularly under the style of domestic architecture in England) are liable to suspicion on this score; and if the fact be ascertained, which is not difficult with the better hygrometers now in use, it becomes expedient in every case to remedy it; either by exposure of a surface of water for the influence of slow evaporation, or by other means. What is merely an inconvenience for the hour or day, may pass into a serious injury to the health, when there is long-continued exposure to it.[1]

The influence of the atmosphere in producing morbid conditions of body, through its *changes of weight*, is a curious subject of inquiry in many points of view. It is chiefly and most familiarly noted, in disturbances of the balance of circulation throughout the body; and particularly in that of the head and lungs; from obvious causes as respects the economy of these organs. The functions of the lungs, indeed, are subject to this influence in several ways. Even the mechanical part of respiration is in some degree concerned; as well as the balance between the external air and that within the bronchial cells, and the relation of the whole to the quantity of blood in the pulmonary circulation:—the action of the heart meanwhile being necessarily affected by what thus disturbs the equal movements of the circulating fluid.

Another consideration again regards the relative effect of air of different density, in producing the proper changes on the blood. It is clear that there exists a point of rarefaction, at which the quantity of oxygen is insufficient for the purposes it has to fulfil. Or, giving the statement its most general form,

[1] In a paper read before the Royal Society in 1836, on the ventilation of the Custom House of London, Dr. Ure states the peculiarities of atmosphere in the Long Room, warmed with hot air, and where 200 persons are always present, to be its extreme dryness (sometimes 70 per cent. of Daniell's hygrometer), and its negative electrical state;—the general effects produced being vertigo, with a sense of fulness and tension about the head; a quick but feeble pulse; and deficient circulation in the lower extremities.

Ermann, in his travels through Siberia, states the dryness of the winds which blow upon Irkutsk from the high table lands to the south, to be such as to cause all the floors and woodwork of the houses to split. An atmosphere thus dry during a considerable period must have some definite effects on the human body.

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there must be a particular specific gravity of air (concurring probably with the medium barometrical pressure), which is best fitted for the necessities of the function; and all deviations from which, in one degree or other, interfere with the completeness of its performance.

These effects, however, under ordinary circumstances, and in healthy state of body, are slight or inappreciable in amount; limited by the range of barometrical variation, and by the usual slowness of the changes taking place. They are augmented of course when the variation is more rapid and of greater extent;—still, however, depending on changes in the state of respiration; and on irregular distribution of blood, from the altered balance of pressure between the external and internal parts of the body. The latter cause might be expected to affect most the vascular system; seeing its structure, functions, and the mechanical principles which in part determine the motions of fluids, even in the vessels of the living body. The common observations with the air-pump and cupping-glasses show the facility with which these vascular textures, and the contained fluids, yield to any such change of balance. And the effects in the diving bell, on the head more especially, produced by an increase of only one-fifth, or one-sixth, in the atmospheric pressure, may be received as proof, though less obvious, of the same fact.[1]

But in less peculiar cases than these, notable effects may occur, when the changes in the weight of the air are frequent, sudden, and considerable, even within the ordinary range of atmospheric variation. Regarding merely the average pressure upon the whole body, it is to be supposed that any very sudden fluctuation (to the amount perhaps of one-thirtieth), may produce temporary changes in the balance of circulation between external and internal parts, of much influence on the latter. And these are particularly to be looked for, when there is individual liability to certain diseases, or close approach to them at the time;—a point requiring to be kept in mind more than it usually is, in estimating the influence of exciting causes, whatever their nature.

[1] The suggestions of Sir James Murray (*Report of the British Association*, 1835) for the use of artificially rarefied or condensed air, in application to the surface of the body as a remedial agent, deserve attention. The cases are numerous, where changes in the local distribution of the blood, thus readily made, might be of much value in the treatment of disease.

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This observation, as I have already stated, appears especially to apply to affections of the brain. I have made note of two or three periods, since I began practice, during which there has been a more than wonted frequency of apoplectic or paralytic seizures within my immediate knowledge; so marked as to make it difficult to attribute the fact to mere casualty, notwithstanding the many circumstances which tend to invalidate such results when not verified by large averages. The same fact, observed by others, has generally been attributed to external heat alone. But allowing what has already been assigned to this cause, the particular character of the weather at these times will scarcely support the inference; nor has the result in question been equally apparent even under higher degrees of atmospheric temperature. While, on the other hand, I have observed at these periods frequent and rapid changes in the barometer, often with great depression of its level; and have noticed at the same time the very common occurrence of lesser affections of the head—vague and uneasy sensations, oppression, vertigo, and what may be termed a feeling of want of proper balance in the frame—all indicating some cause present which tends more or less to disturb the equality of circulation through this organ.[1]

In fact, the ordinary phrases of heaviness and lightness of air (however misplaced or even inverted their use) prove the general consciousness of these changes in their slighter influence on the body. It may be difficult to say through what organ or function this feeling is chiefly conveyed; but probably it is a compound effect of the changes in circulation, in which the sensorium, the lungs, and the muscular system, all participate. Even the organs of digestion seem to be affected, directly or indirectly, by the

[1] Hippocrates mentions the greater frequency of apoplexies in damp and rainy seasons; and Morgagni refers to several other physicians who have made the same remark.

Looking to more recent information in our own country, I may mention the notice taken by different coroners in the month of December, 1839, of the singular frequency of sudden deaths without obvious cause during the first three weeks of this month;—a period when the barometer was generally very low, with almost continual rains, which extended over France, Spain, and even the north of Africa—with much fog, and a singularly oppressive state of atmosphere—little wind, and this chiefly from the S.S.W. or S.E.—the temperature very high for the season. Bowel disorders and low fevers were at the same time very common in London and other parts of England.

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same causes. Without referring to the doubtful instance of vomiting produced in highly rarefied air (of which, however, I obtained some very curious proofs when visiting the Peak of Teneriffe two years ago), I think I have observed frequent disturbance both in the sensations and functions of the alimentary canal under any rapidly diminished weight of the atmosphere, or where its changes were more frequent than usual. I have remarked in another place on certain indications of disturbance to sleep from the same cause.

All these inferences, however, are rendered uncertain by the great difficulty of simplifying the conditions which belong to them, where the physical causes concerned are so unceasingly blended in their operation. It may be, for instance, that what is attributed to changes of weight of air really belongs to electrical changes in the atmosphere, producing or attending the former. Another more familiar case of ambiguity, is that of the sensations experienced in reaching a high mountain-summit. Though often attributed to rarefaction of the air breathed, I doubt not (on my own observation as well as that of others), that they are chiefly owing to the expenditure of bodily power that has been incurred by muscular action, to hurried breathing, and quickened action of the heart. These sensations in great part subside, when the immediate causes of lassitude and disorder are removed. Or, if we yet need explanation of that singular sense of fatigue in the limbs, which is alleged to occur when walking in elevated regions, even without the toil of ascent, we may perhaps find it in a suggestion of Humboldt; whose sagacity is ever awake to all natural phenomena, even such as pass unheeded by others from their seeming familiarity. He conjectures that this sensation may depend on the mechanism of the joints, and equipoise of the bones, being disturbed by the low atmospheric pressure; and the experiments of the two Webers, subsequently made at his suggestion, have afforded a singular confirmation of this idea.[1]

The observation in ascent by balloons, now become so familiar

[1] Poggendorff's Annalen für 1837, No. 1. These experiments made upon the hip-joint after the two bones had been detached by cutting the capsular membrane through, show that the pressure of the air will still retain the head of the thigh-bone firmly in the socket, from which it sinks down when the air is artificially rarefied underneath; the joint thus becoming a sort of air-pump, in which the head of the thigh-bone acts as a piston.

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to us, show, even unexpectedly in degree, the extent to which the body can undergo the most sudden changes of atmospheric weight, without any very obvious effect, where the health is unimpaired, and no causes of bodily fatigue are conjoined. In the note below, I have related some facts derived from the best authority we now possess on this curious subject.[1] It is probable that most of the persons, included under this statement, were in such state of health at the time, as to render them less sensible of sudden barometrical changes, than had they been of feeble respiration, or disordered powers of circulation.

These observations, however, may lessen any surprise at the great powers of accommodation by habit to a constant high degree of rarefied atmosphere. The city of Mexico stands 7460 feet above the level of the sea; and there are inhabited points

[1] I have been recently favored with these observations by Mr. Green, whose boldness and ability as an aeronaut have given him such general and well-merited reputation. Having now ascended in balloons with more than 400 persons, under every possible variation of height, rapidity, and state of atmosphere at the time, his evidence on the points in question is far more complete than any other we possess.

Mr. Green informs me that he has found none of these individuals sensibly affected, otherwise than by the sudden change of temperature and by a noise in the ears, compared by some to very distant thunder; the latter sensation occurring only during rapid ascent or descent of the balloon, and, when greatest in degree, far less distressing than that produced by descent in a diving bell. He has never felt his own respiration hurried or oppressed, except when exerting himself in throwing out ballast, or other management of the balloon, or when suddenly passing into a very cold atmosphere. His pulse is occasionally quickened ten or fifteen beats, but this only when some such exertion has been sustained. He mentions to me expressly, that in no instance have his companions experienced vertigo or sickness; thus rendering doubtful one of the statements current on this subject, and showing how little the two great functions of circulation and respiration are disturbed, under circumstances where much effect might have been anticipated.

Though the inference is limited to two persons, yet it may be worth while to mention the great experiment made by Mr. Green and Mr. Rush, in September, 1838, in ascending to the height of 27,136 feet, or 5¼ miles above the level of the sea; the greatest elevation ever reached by man, and very nearly approaching to the level of Kinchinjunga, the highest ascertained summit of the Himalaya Mountains. The barometer fell from 30° 50′ to 11° during this ascent; the thermometer from 61° to 5°. The first 11,000 feet were passed through in about seven minutes. Yet, under these remarkable circumstances, Mr. Rush suffered no inconvenience but from cold; and Mr. Green little other than from the toil of discharging ballast and gas at different intervals, which hurried the respiration during the time.

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in the Andes of Peru nearly 6000 feet still higher.[1] It must be admitted at the same time that we have no sufficient knowledge of the diseases in these localities, or of the average rate of mortality, to justify inferences as to effects on the body derived from this single physical cause, when forming what may be termed a constant condition of climate.

On a general view of the circumstances stated, there is reason to conclude that the influence of the different degrees of atmospheric pressure in disturbing the bodily functions and general health is rather derived from the frequency of fluctuations, than from any state long continued, either above or below the average standard—that, of the two conditions, suddenly incurred in any extreme degree, the human frame is better capable of withstanding a rarefied than a condensed atmosphere—and that, in every case, the previous health, and proneness to disorder in particular organs, are greatly concerned in determining the results on the body.

Little though its influence has yet been defined, I believe that the *Electrical state* of the atmosphere is that of all its conditions which has most important and diffused effects on the animal economy; more rapid and pervading than any other; and, as one of the vital stimuli, more intimately allied to the functions of the nervous system. It is that, further, which most closely blends itself, either as cause or effect, with all other meteorological changes; producing thereby many of the difficulties already noticed in estimating their relative amount of influence. When modern science has shown us that every chemical action is attended by, if not identical with, electrical change—that the processes of vegetation, as well as those of animal life, involve unceasing alteration in its states—that no two bodies can be present to each other of different temperature, nor even separate parts of the same body be differently heated, without evolution of this agent—that every act of evaporation or deposition of water on the surface of the globe has similar effect of change, even the spray of a waterfall sensibly altering the balance of electricity around it—we may well understand how wide is the circle of these mutual changes, and how impor-

[1] Mr. Pentland, in 1826, ascertained the height of the town of Potosi to be 13,260 feet above the Pacific. Humboldt mentions inhabited places on the Cordilleras at equal elevations.

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tant in the economy of nature, including in this the existence and functions of organic life itself.

It is difficult to advert to the effects of atmospheric electricity on the body, either as a vital stimulus or cause of disease, without noticing the question, whether this great element of electricity is not itself directly engaged as an agent in the functions of the nervous system? If this were eventually determined to be so, the relation of the actions without to those of the same agent within would present itself under conditions still more difficult of apprehension, and little amenable to our present means of research. Hitherto, however, no proof to this effect has been obtained, though much research, and still more speculation, have been directed to the inquiry. The striking results obtained by Matteucci, and those still more wonderful derived from the experiments of Du Bois Reymond, attest the presence of the electric element in the most minute portions of muscular or nervous fibre—its evolution in currents in the act of muscular contraction—and the different direction of these currents in relation to the longitudinal and transverse sections of muscles. Still these researches, however ingenious or subtle in their methods, want an *experimentum crucis* to meet the question as to the identity of the electric and nervous elements. All the phenomena thus exhibited may be explained through the evolution of electric currents by other agents, chemical or mechanical, or by that very nervous element which we are seeking to identify; and we can bring no certain proof that the electricity is not *secondary* in every such manner of its appearance and operation. The whole question is one, the obscurity of which makes it a favorite theme of speculation with those who are ignorant of the true principles of inductive science, and throw dice for discoveries which can only be reached by labor and long research.[1]

Limiting ourselves, however, in this place to the simpler view of the influence of electrical states of air on the human frame, many circumstances occur, well deserving notice, though yet wanting the certainty needful to give them a place in science. The natural history of the animal kingdom through its whole

[1] In my volume on "Mental Physiology" (p. 292, et seq.), I have commented at greater length on this question of animal electricity, and the researches of Matteucci, Du Bois Reymond, Person, &c.

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extent furnishes numerous examples (exclusively of those in which there exists an especial organization for electrical purposes) of the singular susceptibility of different animal species to electrical changes in the media which surround them; and many particular cases of instinct, hitherto unexplained, may doubtless be traced to this source. As a particular instance of such effects, less familiar than many others, is the influence of a thunderstorm on the incubation of eggs.[1] In man the effects are generally less marked, yet nevertheless certain. Without adverting to those singular cases, where the balance of electricity with external objects seems altered by the excess of one of its states within the body, it is obvious that changes of atmospheric electricity have much influence both on the sensations and voluntary powers, producing results variously analogous to those which attend certain morbid states of body more familiar to us.[2] A few may be noticed in illustration from among those most easily recognized.

An atmosphere, proved by other phenomena to be highly charged with electricity, produces in many persons sensations resembling those of slight incipient fever; vague alternations of chill and warmth on the skin, general languor of the frame, debility and aching of the limbs, oppression or other uneasiness about the head. In other instances, the feelings created in the muscles of the trunk and limbs have more of rheumatic character; the resemblance being such as to justify a suspicion I have long entertained that some of the muscular affections, often so termed, are actually derived from this cause. In some persons the susceptibility is so great, that even the approach of thunder-clouds (especially, I think, such as are attended with heavy storms of hail) produces bodily feelings akin to those just described; together with a sense of fulness and pricking about the

[1] This was remarkably testified, in the Exhibition for hatching eggs by artificial heat, during a violent thunderstorm, July 7th, 1839.

[2] Of the instances on record of the curious fact alluded to above, the most remarkable and best attested is that related in the American Journal of Medical Science for January, 1838. A lady without any adequate cause, passed suddenly into a state in which she threw out electric sparks to any conductor around her, sometimes to the distance of an inch and a half, with the ordinary sensations attending electrical action; this state continuing for several months, and subsiding gradually. Other singular details of this case are given, on authority which appears to be good, and without any obvious sources of fallacy.

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eyes, and a slight tingling over the whole body, which I have often noticed in such cases.[1]

The effects of electricity, artificially applied, may be brought into illustration here. The feelings of numbness or aching that remain for some time in the muscles or joints, after the electric current has been passed through them (whether derived from the machine, from voltaic, or electro-magnetic combinations), much resemble those which occur in the early stage of fever, or under other morbid conditions of the body: while the sensations on the skin which some persons feel in the vicinity of a powerful electrical machine in action—or in being electrified on an insulated stool, with much conducting material around—are very similar to others of familiar occurrence, observed especially in certain states of the atmosphere, while electrical changes are going on. And in cases of this kind there is also a certain degree of languor, or even diminished frequency of the pulse; varying in different individuals, but still uniform enough to prove the reality and nature of the effect.

That the effects just cited vary greatly in different individuals, and are very slight in some, is in accordance with what we know of the various action of other atmospheric causes upon different habits. And it may be matter of curious inquiry hereafter, what are the sources of this diversity; and what its influence in giving liability to particular maladies, as well as upon the general functions of life.

One of the best tests of the actual operation of atmospheric electricity on the body is, as I think, that mixed sensation of heat and cold which most persons must recollect at some time to have felt;—or rather, the consciousness of sensations which cannot clearly be defined to be either.[2] Concurrently with such

[1] In the very remarkable storm, which in July, 1839, passed over the south of England, the northern provinces of France, Belgium, and Holland (and probably was still more widely diffused), the sensations experienced by those at all susceptible to them were very oppressive and singular. The precipitation attending the violent electrical changes in this storm might rather be called that of masses of ice than of hail.

[2] It is certain that the sensation of itching depends on several different causes acting on the extremities of the sentient nerves; and it seems probable, from various familiar instances, that one of these is the state of electricity on the skin, in relation to that of the air or particular articles of clothing without. If the assertion of Donné be correct, that there is an opposite electrical state of the two surfaces of the skin, it might lead to further inferences on the subject.

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state of atmosphere, which the thermometer does not in any way interpret to us, there generally occurs more or less of the lassitude before described;—the muscles are readily fatigued; some degree of headache is often present; and other vague uneasiness of the bodily feelings, varying much in different habits, and doubtless influenced by the condition of health at the time.

Though these effects are in general more distinctly experienced previously to, or during, thunderstorms, yet are they sometimes also present in other states of weather where no such storms occur. Certain winds, very common in our own climate, will sustain, even for weeks together, this peculiar character of atmosphere; in degree sufficient to be marked by the results just described, and having still more singular and obvious influence on other animals inferior to man, and on vegetable life. These winds, which may be described, generally, as coming from all eastern points of the compass, but more especially from the quarter lying between northeast and southeast, deserve inquiry under all the aids which modern science can afford. Their various effects on the human body, and on all living organization, are in no wise explained by the temperature or weight of the air. The great dryness of some easterly winds may give better reason for certain of the phenomena, but will scarcely explain the peculiar sense of muscular aching, uneasiness, and languor they produce in many habits; the almost instant perception of their effects by some persons, even without any exposure to the external air; and as rapid consciousness of change when they cease. Such sensations belong much more to what we know of electrical agency than to any other cause we can assign; but they need observations more exact than have yet been made; and a careful comparison of these with the physical properties of the winds in question, which future research may also better determine.

Whatever the natural causes which render some of our easterly winds thus peculiar, that from the southeast may certainly be considered to have direct connection with the Sirocco, as it sweeps with greater or less intensity over the southern half of Europe. This very singular atmospheric current, which on its more distant border has probable relation to the Simoom and Harmattan, the hot winds of the African desert,—and passes over the Mediterranean Sea under the names of the Levant wind

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and Sirocco,—reaches England on the opposite side; with its peculiar qualities much mitigated; yet still showing the same origin in its general direction, in its hygrometrical conditions, and in what I believe to be its electrical influence on animal life. No sufficient explanation has yet been given of these peculiarities, nor are they perhaps definite enough as facts to warrant much theory on the subject. I cannot doubt, however, from my own observations, that the electrical state of these great atmospheric streams, whencesoever derived, is that to which their effects are mainly due. I have witnessed in different parts of the Mediterranean such singular and repeated proofs of this as to give assurance of the general fact, though there are yet wanting the exact determinations required to fix its place in the history of physical phenomena.[1]

[1] In my Travels in Albania, &c. (2d edition, 1819), I have related two occurrences of this kind, in which the electrical phenomena attending the Sirocco, as seen at night, were very striking. One of these instances I cannot readily forget. It was when approaching the coast of Ithaca, under the obscurity of a dark evening in October, after a still and sultry day, the wind arose suddenly and strongly from the southeast, kindling almost instantly every part of the sky with gleams and flashes of electric light; vivid enough at intervals to allow the reading of the smallest print; and giving outline not only to the cliffs of Ithaca closely above us, but even to the distant mountains of Santa Maura and the Acarnanian coast. As in almost every instance in which I have witnessed such appearances from the Sirocco, dark masses of clouds speedily began to gather in the northwest, the quarter towards which the wind blew; and amidst these the electrical discharges or interchanges were singularly splendid in effect, continuing till a late hour in the night. No thunder was heard during any part of the time, nor did rain fall; but I found, from observations on this and similar occasions, that the quantity of moisture in the air was very great, and have generally noticed the wind to subside with a fall of rain.

In other instances, where the Sirocco began to blow in the evening or during the night, I have observed appearances of the same kind, though none equally striking. They are generally, according to my experience, more marked at the beginning of the wind, as might be presumed likely from the blending of different portions of atmosphere unequally charged with electricity or in different states.

In the work just alluded to, I have mentioned the position of Malta as very favorable for a series of observations on this remarkable wind. The best information we yet have from this island is that given in Badger's History of Malta. The effects of the Sirocco on the barometer and thermometer require to be noticed: still more those which indicate its electrical and hygrometrical states. The latter, too, need the comparison of inland observations (which must be obtained elsewhere) with those made by the sea-side. Though Dolomieus's assertion, that the atmosphere of the Sirocco contains less than the due proportion of oxygen, is liable to

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Our knowledge of atmospheric electricity is, in truth, still in its infancy. The researches of Peltier, and the more recent and remarkable results obtained by Quetelet, from five years of continuous observation at Brussels, have indeed determined much as to the statical condition of the electricity of the atmosphere, and the changes it undergoes at different seasons of the year, at different times of the day, and under different hygrometrical states of the air. But the causes of production, distribution, and change;—its relations to that electricity which circulates in magnetic currents, or otherwise appertains to the earth, or may possibly exist in space beyond the atmosphere; its connection with atmospheric heat, moisture, or weight; with the formation of clouds, and the phenomena of wind, thunderstorms, and rain; and above all, the true theory of its positive and negative states,—each of these conditions is still largely open to inquiry. The latter especially, which has most assured and closest relation to all the rest, is the great mystery still hanging over electrical science; the solution of which would not merely determine these particular questions, but probably, in its connection with the general doctrine of polarity, enlarge our whole view of the attraction and combinations of matter, whether in atoms or masses, throughout the universe.[1]

What has been thus far said regards chiefly the influence of electrical states of the atmosphere on the sensations and muscu-

much doubt, yet ought the air to be examined eudiometrically also; and particularly as to the proportion, if any, of Ozone contained in it. And to these notices should be added others, as to the frequency, periods of occurrence, and duration of the wind;—the points from which it blows, and the changes in these;—the external appearances attending it;—and its effects in producing bodily disorder, whether of the sensations, voluntary power, or vital actions.

A conjecture of Colonel Reid, in his very valuable work on the Law of Storms, would, if verified, afford a plausible account of the properties of this wind, hitherto so little explained by any local circumstances. He supposes, in conformity with his general doctrine, that the Levanter or Sirocco may be the southwest wind of the great Desert of Sahara, sweeping round so as to become an easterly or southeasterly wind in the Mediterranean, and still retaining qualities which it has acquired in passing over this vast region of sand. Any future observations in Egypt, or other eastern parts of Africa, which might show strong southerly winds, corresponding in time with the Levant winds of the Mediterranean, would furnish much confirmation of this conjecture.

[1] I may notice here the remarkable fact stated by Quetelet, that in daily observations for five years, the electricity of the atmosphere was only found *negative* in 25 instances; all these occurring in connection with storms.

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lar powers. Unless justified in considering as such the occasional effects of lightning on the body, I know no express example of disease which we can affirm to be produced by this agency. Some authors, indeed, have attributed to it certain epidemics of singular character, and not easily referable to any known cause. But in this opinion they have hardly defined, whether it is to be considered as directly producing the disorder, or merely a state of body predisposing to receive it; leaving open still the third contingency of its simply evolving from other sources the virus or material cause of disease. I have elsewhere shown that it is difficult, if not impossible, to connect these erratic disorders with any state of weather or known quality of the atmosphere; and the reasons derived from their history apply as distinctly to electricity as to any other property of the element which surrounds us. We must, however, admit the possibility, both as to these and other disorders, of the two latter contingencies just stated. Electricity may be concerned in favoring the generation of malaria, whatever its nature; or it may induce a state of body more liable to be affected by this, or by other causes of disease in activity at the time. We have no proofs on which even to approach towards assurance, but presumption from several sources that this great agent cannot be wholly inert as respects either of the conditions in question.

Though unable then to affirm any one disease to be actually produced by electricity, yet, considering the subject in its whole extent, it is impossible not to see the likelihood of its influence on the body in many ways hitherto undistinguished, or not understood. If a stroke of lightning can in an instant destroy muscular irritability throughout the system, and prevent the natural coagulation of the blood, either directly or indirectly by hastening putrefaction—it is clearly to be inferred that lesser degrees of the same action must have definite effects, bearing proportion to the intensity of the electrical changes or transferences taking place.[1] The conclusions, best warranted by the facts we possess, would direct us towards the blood and nervous system generally, as the parts of the animal economy most liable to be thus affected. The influence of atmospheric elec-

[1] Though these effects of lightning are generally recorded, and in part on the authority of Hunter, there is still some ambiguity attending them. See the Memoir of Dr. Davy on this subject in his Physiological Researches.

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tricity on the latter is shown in the various effects already mentioned on the sensations and muscular power; and the proof is greatly strengthened, though indirectly, by the numerous experiments which prove the influence upon these two functions, of electric action from different sources, applied directly to the nerves themselves.[1] The quantity or tension of the agent, as affecting the body through the air, may be less, and its application not so direct on the nervous system. The low average intensity of animal electricity, as ascertained experimentally, must also be taken into account. But with all these allowances it is impossible that the effect should be wholly absent or different in kind; and circumstances may often greatly augment its degree, disordering in the same ratio that balance which is most conducive to the general well-being of life.

The same reasoning applies equally to its influence on the blood; and though this part of the subject is even more obscure, yet is there presumption that here the effects occur which are of greatest import in the history of disease. All that chemistry has recently done to determine the nature and relation of parts in the blood (concurrently with that great fact which has now been established of the identity of electrical and chemical action) justifies the belief that every material change of balance between the electricity without and that within the body, must have effect on the state of the circulating fluid—transient and wholly inappreciable, it may be, in the great majority of cases; in others, possibly, of longer duration and more extensive in degree. The general relation of acid and alkali, as important in the chemistry of life as in that of inorganic matter—obvious, not only in the blood itself, but in the materials and processes by which it is formed, and in the secretions and excretions derived from it—this relation is one in which we have peculiar and constant evidence of electrical agency. The coagulable property of the blood, in whatever it consists, is closely affected by the same cause, even when acting through various intervening tissues. Though we have no equal proof as to the globules, yet their definite form, size, and other peculiarities (necessary as it would

[1] The researches of Humboldt, Müller, Prevost and Dumas, Dr. Wilson Philip, Becquerel, and other physiologists, might be referred to in this place—not equally certain in results, nor conducting their authors to the same conclusions; but concurring to show the remarkable nature of this agency as a stimulus on the nervous and muscular systems, if indeed it be nothing more.

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seem to the existence of each species), make it probable that they are liable to alteration from an agent, which seems more than any other to determine all definite combinations and changes in the material world.[1]

The tenor and extent of the argument here must be rightly understood. We have no proof of the action of atmospheric electricity, in any of its ordinary states, upon the blood. But the effects of lightning, and the influence of the same principle, proved by experiment in other modes of application to this fluid, warrant the belief that such action may exist; and, if existing, that it must be a frequent cause of disorder throughout every part of the animal economy. We cannot trace diseases with certainty to this source, but how rare are the instances in which we can affirm their real causes! The actual void of knowledge justifies our seeking them through all the new agencies which physical science may disclose; and none is more likely to afford successful results than that now before us.

Two classes of facts, neither of them yet sufficiently examined, are obviously very important to the inquiry. The first includes the indications which diseases themselves may give, in their progress, of alteration of electrical state in the body. The second involves the more general question as to the development of electricity in the animal frame; its natural variations from age, sex, temperament, and connection with particular bodily functions; and its manner of relation to the electricity of the air without. With the exception of some curious observations of Humboldt and Pfaff on the electrical state of rheumatic patients, we have nothing that approaches to certainty on the former subject. On the latter we possess more results, but all requiring revision and extension by further experiment. We still have no averages sufficient to show the relative frequency of positive or

[1] The action of electricity on the blood has been the subject of the same zealous research as its influence on the nervous system; and directed ultimately to the same question, whether it is not itself the most essential principle in this fluid, ever present, and determining all the changes which take place within it? The reasoning of Müller (*Handbuch der Physiologie*, p. 128,) in refutation of the singular experiments and inferences of Dutrochet, may be consulted as an example of the ingenious and minute inquiry given to this subject, as well as of the ambiguity which still surrounds it; both as respects the chemical changes, usually so termed, in the blood, and the action of voltaic electricity in promoting or modifying them. The question concerning the relation of animal electricity and animal heat is a very curious part of this subject.

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CHAPTER VIII.

ON DIET, AND DISORDERS OF DIGESTION.

Looking over the notes I have made on this subject in the course of practice, I find them in various parts superseded by the many and valuable treatises which have been published of late years. These works, founded upon an improved physiology of the organs of digestion, have done much to give dietetics the character of a branch of science, which it certainly did not possess before.[1] Even now it may be doubted whether our materials are ample or exact enough for methodical arrangement; and whether many of the rules founded on them are not calculated to foster, rather than repress, the vague and changeful fancies which prevail on this subject. It is certain on experience that no proportionate benefit has yet been obtained from all hitherto written upon it. For this there may be many causes. One doubtless is, the difficulty of establishing or applying positive rules for conditions so infinitely varied, that the evasion of these rules becomes easy, or is even apparently sanctioned by the peculiarities of each case. Nor can it be affirmed that the precepts of medical authors themselves are concurrent on the principles of diet, and the management of dyspeptic disorders. Many points are still disputed: and on few perhaps are the criteria of truth so complete as to possess the influence that is required for their successful application to practice.

These circumstances, and still more the habits of society

[1] I may mention particularly the works of Dr. Paris, Dr. Combe, Dr. J. Johnson, Dr. Wilson Philip, and the article on Indigestion, by Dr. Todd, in the Cyclopædia of Practical Medicine. The physicians of the United States, who now contribute so eminently to medical science in all its branches, have added to our knowledge on this subject. And their writings have the greater interest, as stating the result of manners of life differing in several respects from the ordinary habits of our own country, and of other parts of Europe.

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among the higher classes, and the influence of dyspeptic complaints on the minds of those affected, render the treatment of such disorders a matter of great interest, even in a moral point of view. They unhappily furnish an arena on which all the worst parts of medical practice find their readiest display. Fraud, intrepid in its ignorance, here wins an easy triumph. Seconded on every side by prejudices, fashions, and foibles, and taking advantage of the mind and body in their weakest and most sensitive mood, it deals out precepts and drugs with a pernicious facility;—sometimes altogether at random;—sometimes, and even more injuriously, with one common scheme of treatment applied to the most variable and incongruous symptoms.

These abuses indeed, in their worst form, exist only on the outskirts of the profession. But it will be admitted by all who have candor and experience, that there is no part of medical practice where knowledge and good faith are put to equal trial as in the management of dyspeptic complaints. Even the effect of the disorder in obscuring the judgment, and rendering impotent the will of the patient, becomes an embarrassment to the physician. If his own judgment be slow and wavering, he is deprived of aid—if hasty and rash, of that control from the opinion of his patient which is frequently needful. The mind of the dyspeptic is uncertain and fickle. He interprets falsely his own sensations, and the effects of the treatment employed; is unduly confident at one moment and under a new remedy; at another time as irrationally desponding;—prone, moreover, to change his medical adviser, and to resort to any person or remedy where there is larger profession of relief.

All these things, familiar in practice in this country, make the situation and conduct of the physician in cases of dyspepsia hardly less difficult than in acute and dangerous diseases. Though the symptoms before him are not so critical in kind, they need sound moral management as well as discreet methods of medical treatment.[1] Forbearance and firmness are both required; and together with these, integrity and good faith. The admirable precepts as to uprightness in practice, which came

[1] What Aretæus says regarding the treatment of chronic diseases in general, applies singularly to dyspepsia, as one of the most frequent of them. *Ενθα δη αρετη διαειδεται ανδρος ιητρου, και μακροθυμιης, και ποικιλιης, και χαριτος αβλαβυος των ηδεων, και παραψαστος.*

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down to us under the great name of Hippocrates, obtain here their closest application; and may well be impressed upon all who are entering on a medical life. The mind must be fashioned early and strongly in these professional principles; as they are rarely attained afterwards, and not always easily preserved, amidst the many difficulties which beset the conduct of the physician.

I dwell on the foregoing points the rather because experience has convinced me of the great abuses existing in this part of medical practice, from the excessive employment of medicines, where management of diet and habits of life would be of far greater avail for good. These abuses have been much lessened of late years; but enough still remains to justify a reference to them, and the demand for their further control. I cannot doubt that to this cause in great part may be attributed the growth and continued prevalence of Homœopathy in England; while it has passed into comparative neglect in the country which gave it birth. The love of novelty, and of all that is secret and unintelligible, doubtless contributed to foster this as it has done so many prior fashions, calling themselves systems, in the treatment of disease. But the contrast of the occasional Homœopathic globule with the daily series of pills and draughts, affords more entire explanation of the fact. The public at large, little capable of judging rightly either as to the causes or treatment of disease, accepts as a boon the practice which exempts them from unceasing physic; and attributes to the infinitesimal dose effects which are really due to this exemption alone. Such is especially the case in dyspeptic disorders, where the malady is fed by excess of medicine, and often greatly mitigated by its removal. And here, accordingly, Homœopathy, inefficient in all acute diseases, has found a wide field in which to work; the abuses which had debased this part of practice, giving a specious sanction to any new system, free from similar reproach. I am persuaded that the experience of those physicians, who have been most observant of the medical history of the last thirty years, will be in accordance with this view of the subject.

For the reasons already named, I shall not seek to give any method to the remarks which follow respecting diet and dyspeptic disorders. They are drawn from my experience, on no

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other principle of selection than that of appearing to be least regarded in proportion to their practical importance. If desultory, it may be truly affirmed that the subject to which they relate is so likewise. Even the term of dyspepsia is variously and vaguely used by medical writers themselves; and in truth is not readily defined, where so many organs are concerned, and such various causes of disorder. In a formal treatise, this difficulty requires to be met by some sort of arrangement of the cases and causes in question. It is not necessary here, where the object is limited chiefly to practical views, and those of a general nature.

It is needful to remark in the outset, that rules of diet may be pressed too closely, as well as too vaguely, in the treatment of disorders of digestion. Such rules may come to exercise a morbid influence over the mind of the patient, counteracting all the good obtained. Attention to his sensations is already too much awakened in the habitual dyspeptic; and often the greatest benefit which can be conferred is that of weaning it away from them. In another place I have alluded to the singular influence of the mind, simply directed to the digestive organs, in altering or disordering their natural functions; and this even where no morbid solicitude exists on the subject.[1] Every such effect is greatly aggravated in the case of the dyspeptic. His minute and anxious watch over his sensations generates the very evils which it is the object to remove. A system of rules for diet and other management of the digestive organs may be necessary in such cases; but their effect on the mind must be observed, to see that they do not usurp too much upon it, and sanction injuriously those morbid habits which, arising out of the malady, augment the symptoms and retard their cure. A strict rule, good in itself, may often expediently be relaxed or dispensed with, if found thus to influence the temper of the patient. Austerity of system must bend before the variable conditions with which we have to deal in the treatment of disease.

Many points of practice connect themselves with this state of mind in dyspeptic disorders. It may be well, for instance (though the rule is by no means an invariable one), that the bowels should act each day in such cases. But if the patient

[1] On the Effects of the Attention of Mind on the Bodily Organs. *Chapters on Mental Physiology.* Chapter II.

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becomes morbidly intent on this object, as often happens, all value of the habit is lost in the anxiety it creates, and mischief is generally incurred in the means employed to promote it. Here every effort must be made to disengage his mind from the presumed need. It is often even expedient for this purpose expressly to advise that there should be action on alternate days only; gaining thereby the chance of some interruption to these habits of morbid feeling. The disorder they produce in the various parts and stages of digestion does generally itself, indeed, prevent that regularity which is so anxiously sought for.

So also it may be very desirable that the dyspeptic should dine at regular and reasonable hours, and from a simple and discreet table; but if this rule brings him to a solitary meal, set apart for his express condition, more of ill than of good is usually the result. It is rarely expedient that he should feed alone. His mind needs to be solicited away from attention to the functions and feelings of the stomach; and this can only effectively be done by society at the time of eating. The apprehension of indigestion creates it;—sometimes almost as an instant effect. When the stomach is full, the less the mind has to do with it the better. In this fact, as well as in the hurried dealing with food while alone, we find explanation of what is often cause of surprise to the dyspeptic, that he feels better after the indulgences of a large dinner party, than in the seeming discretion of his solitary table. We are often then obliged to strike a balance between these evils, and to sacrifice something of principle for a practical expediency of greater importance.

Upon the same principle it is obvious that rules of diet lose much of their value in practice, if too various and minute. This applies alike to the treatment of actual dyspepsia, and to general methods for preserving a healthy digestion. Multiplicity of rules may become as ineffectual as no rules at all. We are bound to take human nature and society as we find them; amending if we can, but not, through aiming at too much, forfeiting that which may be gained by more simple and compendious means. The dyspeptic patient feeds his disordered fancy on minute maxims, adopting and dismissing them with equal facility and forgetfulness. The patient who seeks only a rule by which to preserve health, disregards a series of small observances, which he feels

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as burdensome; and which in truth, if strictly observed, often become an unwholesome restraint.

While difficult then to say how far rules should go, so as to attain the greatest amount of practical good, the physician must be furnished with full knowledge of all that are sanctioned by reason or experience, in order to give them direction to particular cases. For the guidance of patients themselves, those of course are best which are most promptly and safely applied; neither harassing the mind by anxieties of choice, nor the body by encouraging wayward fancies as to methods of prevention or cure. If, for example, I were to specify any general maxims as to food, preferable to others from distinctness and easy application, and serving as a foundation for lesser injunctions, they would be the following:—

First, that the stomach should never be filled to a sense of uneasy repletion. *Secondly*, that the rate of eating should always be slow enough to allow thorough mastication, and to obviate that uneasiness which follows food hastily swallowed. *Thirdly*, that there should be no urgent exercise, either of body or mind, immediately after a full meal.

The simplicity and familiarity of these rules may lessen their seeming value; but in practice they will be found to include, directly or indirectly, a great proportion of the cases and questions which come before us. And many such questions, as for example those which relate to different qualities of food, would lose great part of their difficulty, were these maxims successfully enforced. When the quantity taken does not exceed the just limit;—when it comes to the stomach rightly prepared by mastication, and by admixture with the secretions of the glands which aid the first stage of digestion;—and when no extraneous interruption exists to the proper functions of the stomach in this stage, the capacity of digestion is really extended as respects varieties of food, and tables of relative digestibility lose much of their value.

It is certain that different temperaments require, whatever be the causes of this diversity, different proportions of aliment; and the same constitution alters its demand at different times, both in health and sickness. No rules of diet, therefore, can be made positive as to quantity; and the attempt at such is now generally abandoned. The criteria to be taken for particular cases are

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those furnished by the state of the several organs concerned in digestion, both immediately and at various intervals after food; and also by the effect of digestion, while in progress, upon other functions of the body. This is for the most part too intricate an observation for the patient himself. It is enough for him (and much indeed of positive good) if his adherence on conviction can be obtained to the simple rule stated above; viz., habitually to stop eating at a point short of uneasy repletion. In effect, this maxim is nearly identical with the more familiar one of not eating beyond the appetite. But it is to be preferred for practical purposes, as being less liable to ambiguity and self-deception. Though with apparent relation only to the first stage of digestion, it is in truth more or less a provision for every succeeding part of the process, and for the well-being of all the organs concerned in it. A meal, which sits lightly on the stomach in the outset—producing during the first hour no oppression, drowsiness, heat, thirst, acidity, or other uncomfortable feeling—will probably pass healthily through all succeeding stages of digestion. If otherwise, the evil arises from some accidental or especial cause, against which no ordinary rule can provide.

The symptoms just recited, though perchance slight and transient in each particular instance, yet are readily recognized both in health and sickness, and may fitly be taken as suggestions for the future.[1] Such effects may be owing either to excess in quantity, or to error in quality of food. When there is doubt as to this point, it is for the most part simpler and shorter in practice to act upon the former view, and to lessen experimentally the quantity of food taken. If the object be not thus wholly attained, the evil is at all events diminished in amount, and better indication given as to the real cause of mischief.

That there is a general excess in quantity of solid food among the higher classes in this country, perhaps in all highly civilized countries, may be admitted as certain. In its effects, direct or indirect, it is one of the circumstances which most tend to equalize the actual condition of different classes. No reasoning or quotation of cases is needful for the proof of the mischiefs hence derived. Daily experience is the argument here; both as

[1] Better and more succinct description of them cannot be given than that of Hippocrates. *Οἱ μεν γαρ ην αριστησωσι μὴ ξυμφεροντος αυτοισιν, ευθυς βαρεες και νωθροι το σωμα και την γνωμην, χασμής τε και νυσταγμου και διψης πληρεις.*

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to the effects of single excesses, and of that smaller habitual excess, which, little felt at the time, does nevertheless accumulate a store of ills for the whole of life, affecting in various degrees all the functions both of body and mind. The observations of Dr. Beaumont, under the singular facilities afforded in the case of St. Martin, add more direct proof to this ordinary experience. In showing to the eye the immediate effect of excess of food, as well as wine, in producing an erythematous state of the mucous coat of the stomach, they render more explicit the influence of such excesses long continued; and the passage from slight and transient disorder to permanent disease of texture. These living observations have a value tenfold that of any attainable in other ways; and especially as applied to an organ of such complex condition as the stomach, so variously acted upon by mental as well as physical causes.

The vague or mistaken use of the word *debility* is the source of much mischievous error in the management of diet; both in acute disease, and in the ordinary treatment of dyspeptic cases. It behooves the physician to keep this constantly in mind, lest his own judgment be misled by the wrong interpretation of others. The term in question is often applied in cases where the sensations so described are, in reality, owing to *oppression;* and oppression from that very food which is unceasingly sought for as a remedy. The organs of digestion are overloaded; and not only their powers, but those of the whole body, thereby weakened and impaired. And to the condition thus produced we give the same name by which we described the effects of excessive hemorrhage, of typhus fever, of pulmonary disease, or other protracted disease, exhausting all the powers of life! The whole subject of debility, in its more general sense, physiologically as well as practically considered, is of great interest, and merits the most careful examination, freed from those ambiguities of language which old medical controversies and the popular prejudices of every age have alike contributed to engender.[1]

[1] Those who are familiar with medical literature will remember the long and angry controversies which have existed, almost in our own age, on the question of *direct* and *indirect debility*—disputes in which words, without due definition of meaning, have "mightily entangled and perverted the judgment," and led to frequent and serious errors in practice. The best excuse for these controversies is to be found in the intrinsic difficulties of the subject. Debility scarcely admits of a single

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The proper regulation of diet under actual disease is not, if rightly viewed, so difficult a matter as it might seem from the infinite variety of conditions with which we have to deal. The great law of the living economy comes in here for our guidance —that the appetite for food is lost or diminished, in exact relation to the impairment of the powers by which it is digested and converted to the use of the system. The exceptions to this law (many of them such only in appearance) are too inconsiderable to require enumeration. Its truth is singularly denoted in all cases where there is febrile action present in the body; and its generality gives it great value as an index of disorder, as well as of the right methods of dietetic treatment.

I have mentioned elsewhere the aids we are entitled to draw from the patient himself, in this point of practice, where disease has restored nature for a while to its dominion over the appetites. It is rare that any mischief can arise from following this guidance, if carè be taken to ascertain its reality. Nevertheless, the physician is liable here to various difficulties, created chiefly by the mistaken importunity of those around the patient. No conviction is harder to be removed than that which regards loss of appetite as itself the disorder—food as the proper and certain remedy. Even the concurrence of the patient's feelings with the judgment of the physician is scarcely of avail against this persuasion. And when these are altered in part by returning

definition. It can only be defined by a description, including several different states of body, partial or general, in which a certain common condition is found, though the cause producing such states are of very various nature. This common condition may best, perhaps, be described by the simple phrase of *loss of power*—a phrase involving no hypothesis, and expressing well the main fact that pervades all the forms, and illustrates the causes, of real debility, as a state of the living body.

There would be great value in a good treatise on this subject. But, to be complete, it must go deeply into these causes and forms, designating not only what is partial or local, what general debility, but further also, distinguishing clearly the causes concerned in both. Such are the debility of natural temperament, or a want of power proportioned to the bodily frame, existing from birth and pervading life—that, transient in kind, arising from an excess of use of the bodily powers—that consequent upon fever and other bodily diseases, including those of the brain and nervous system—that induced by physical agents, comprising even states of the atmosphere around us—and that proceeding from various emotions and moral causes affecting the nervous power. The mere statement of these several conditions (and others might be cited) show their singular complexity considered physiologically; while the experience of every medical man will testify to the supreme importance of a right discrimination of them, for the safe guidance of his practice.

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health, and old appetites and habits begin to recur, the risk of excess becomes greater, by the enfeeblement of that natural control which is the best security against it.

The adult patient, however, is better able than the child or infant to protect himself against such hurtful importunities. In the case of the infant, more especially, a facility is often afforded to the commission of this mischief by the thirst of fever, which allows liquid food to be largely given, and under the deception of appearing to be taken eagerly, when most unfit for the disorder that exists. These evils are familiar in practice, and not easily obviated altogether. They may best be diminished by consistency and firmness in the medical man, whose unwilling concessions in a few instances weaken his influence in all.

As to what regards our second point, the *manner of taking food*, little can be added to the maxims which are familiar on this subject. The recent experiments of some German physiologists on the properties of acidified mucus and other products from mucous membrane, as agents in digestion, gives new proof of the necessity that the food should be well mixed with the saliva and secretions of the mucous glands, in the early stages of this process.[1] And the observations of Dr. Beaumont (aided by the facilities just mentioned) on the secretion of the gastric juice as an effect of the stimulus of food received into the stomach, and on the peculiar manner in which its inner coats contract on each fresh morsel, show the necessity of allowing due time to these operations, and not disturbing them by matters hastily swallowed and imperfectly prepared. In youth, the activity of the system, seeking fresh materials for growth, and for other remarkable changes then in progress, readily overcomes all difficulties. But at more advanced age, and when these demands are diminished or have ceased, the stomach needs to be dealt with more leisurely and gently. And at all times of life, common experience tells us of the sort of convulsive reaction, weight, and other uneasiness produced by hasty swallowing and hurried meals. This, in fact, is one of the many causes of dyspepsia to those of sedentary habits who feed alone; and not less to those who eat, as they live, in a tumult of business, measuring their meals by minutes

[1] I allude to the research of Eberle, Schwann, and Müller, which have been repeated and extended by Dr. Todd in this country.

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stolen from the labors of the day.[1] The dyspeptic often pleads to the physician his solitary dinner, and plain joint of meat, as things that ought to protect him from his disorder;—ignorant, and hardly to be persuaded, that these are the very conditions which augment and protract it.

It is another suggestion of prudent experience, that meals of animal food should not be admitted to crowd too closely together. The function of digestion as respects each organ is one of stages, and does not allow this forced interference of one stage with another. Animal food, of ordinary cookery, especially requires this caution; and the meat luncheon and dinner, too closely brought together, are causes of mutual disorder to the digestion of each.[2]

With these considerations is connected our *third rule*, that there should be no sudden or urgent exertion soon after a full meal; nor indeed immediately before it; for the same general reason applies to both cases. The stomach requires (as does every organ for its appropriate function) a sufficient supply of nervous power, whencesoever derived; and a proportionate increase of blood in its circulation, to minister to the actions of which digestion is the result. It may be a physiological fact that these two conditions are identical; or that one needfully involves the other. But whether so or not, it is equally certain that both the nervous power, and the blood needful to digestion, are diminished and disturbed by strong exercise immediately before or after a meal. And to this cause of disorder must be added in the latter case, the effects of mechanical agitation, which is no doubt often concerned in disturbing the process,

[1] In Thomas Paynell's *Regiment of Health*, 1557 (a translation of the Regimen Sanitatis Salernitanum), this matter is quaintly but well expressed :—" Prolongynge of tyme in eatynge moderately (as an hour's space), to chawe and swalowe our meate well, is aloweable, and helpeth moche to the conservation of health. For good chawinge and swalowynge downe is as halfe a digestion. And ill chawinge doth eyther let dygestion, or else doth gretly hyndre it. But prolonginge of tyme in rallying and tellyng of tales two or three houres is ryghte hurtful." And the reason for the latter clause is just, though that of probable excess of quantity in such cases might also be added. " For when the last meate is received, the first is well neare dygested. Therefore the said meates in divers of their parties, as touch ing dygestion, be not like."

[2] Regarding the relative digestibility of different kinds of food, the best information is that of the tables given by Dr. Beaumont; though some ambiguity must still be admitted to exist on the subject.

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either by interference with the chemical changes essential to it, or by preventing that remarkable distribution of aliments within the stomach, the result of powers we cannot yet pretend to define.

The proofs of these facts are furnished by constant experience, and are familiar to us amongst other animals; yet is attention not sufficiently given to them, either in the habitual directions of physicians, or in the rules which men themselves apply to the management of their diet. Hard exercise and fatigue are often understood as a sanction for immediate and ample food, without regard to the expenditure of power that has taken place, or to the direction which the circulation has got towards the muscles and capillaries of the skin. Those who are exposed to the necessity of long and fatiguing journeys speedily learn the error of this. But experience of such kind is generally needed to teach it; nor is this always sufficient against the force of early impressions, and the faulty habits of society.[1]

What is true as to bodily exercise after food is almost equally so as to mental emotion, or intellectual labor. Neither with sound nor unsound digestion ought there to be any intent exertion of mind at this time. Strong or sudden emotion, from whatever cause, will instantly disturb or even stop the whole function; and short of this there is every degree in which the mental feelings habitually derange it. To this cause we may in some part attribute the frequent occurrence of dyspepsia in middle life; when the excitements are more active and various, and the passions inflict greater disturbance on the body. Their influence is familiar to observation in the sensations of the moment; and not less in the symptoms which arise from repetition of the disturbance. Two or three days of continued anxiety will bring disorder and debility into all the actions of the digestive organs, however healthy their previous state.

But even urgent intellectual exertion, unattended by any emotion (unless a laborious effort of the attention be so termed) interferes for the most part with easy and perfect digestion. The precept of Celsus, "*sin lucubrandum est, non post cibum id*

[1] I know from my own observation how well Government messengers, and the Tartar couriers of Turkey, have generally learnt this lesson; derived from the necessities of their manner of life, which are far more effectual for teaching than any other maxims. We apply the rule carefully and extensively in the management of our horses and dogs, without rightly heeding it for ourselves.

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faciendum, sed post concoctionem," will ever remain good. We have in this a particular example of that remarkable law, which stands at the threshold of every theory of life, viz., that one function in vigorous or excessive exercise diminishes more or less the power of simultaneous exertion of any other. One must give way, or both will be imperfectly performed. The experience of every man, and of every day, gives proof of the universality of the fact.

We may, as before, seek explanation of this, by estimating the common nervous power as a given quantity at each moment of life; capable therefore only of a certain amount of effect, however directed, and requiring replenishment when exhausted. Or we may look otherwise to the quantity of blood required by every organ for the complete performance of its functions; the due distribution of which, according to this demand, is expressly interfered with in the case before us. But whatever the manner of viewing the subject, it is certain that this is a point of importance to good digestion, and that it ought to enter into every rule of treatment for disorders of the stomach. And the rather so, as the dyspeptic is often by temperament prone to what may be termed intellectual excesses, as well as to earnest and easily excited moral feelings.

The relation of digestion to sleep is one of the most important in the economy of life. These two functions severally aid or disturb each other, in proportion as they are perfect or imperfect in their course; and no rules are more important than such as apply to this relation. It must be admitted, however, that all such rules are exceedingly scanty and incomplete, notwithstanding the perpetual experiment which life affords on the subject. It is difficult to extricate clear results from so great a variety of conditions; and unhappily the current fashions and phrases tend rather to perplex than inform our judgments on the subject.

Setting aside the effects of particular articles of diet, which it is needless here to specify, the practical questions chiefly regard the relation of time and quantity of food to sleep; and these are the fitter objects of study, as some averages are attainable and convertible to use. It is clear, for example, from observation both of man and other animals, that a certain quantity of food in the stomach, concurring especially with the habitual

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time of rest, tends to produce sound and healthy sleep;—that an excess in quantity brings on such as is broken, uneasy, and oppressed;—while sleeplessness is usually the effect of the stomach being empty and needing support. To these general facts, without inquiring into their physiological cause, may be referred most of the particular relations between sleep and food, and the precepts founded upon them. The fashions of society infringe largely upon the principles thus established; and though the powers of habit and accommodation in the system do much to lessen the evils that result, yet it is certain that some methods are greatly preferable to others, and that observation cannot better be employed than in ascertaining these.

An important circumstance, in reference to sleep, is the relation between the principal meal of the day and the time of going to rest. This is a question of constant occurrence in practice; and, without passing into details, it is enough to say that much may be done for the restoration of sleep, where this is at fault, by altering the time of dinner to an earlier hour; so as not only to admit some bodily exercise in the interval, but also a light supper before going to bed.[1] The benefit of such change from the ordinary usage is often immediate; depending partly on the avoidance of the recumbent posture, and of efforts to sleep at a period of digestion unfit for this state; and further, and not less, on the direct effect of moderate repletion in disposing to rest. The latter circumstance, again, may depend in part on the mechanical influence of such repletion; principally, perhaps, on its furnishing material for the secretions of the stomach to act upon, which otherwise fret with uneasy sensations the nerves of the organ itself. A small quantity of acid formed in digestion, and remaining on the stomach, will often suffice to prevent or disorder sleep, and to disturb the sensations of the whole body;—the proof of this being the degree of relief from a slight dose of some alkali taken at the time.

Various other suggestions will occur in practice, founded on the facts just stated; some of them, though simple, of greater value in the maintenance of health, and treatment of disease, than might be judged likely from their scanty employment. I would mention, as an instance, the aid that may often be got to

[1] It is a somewhat curious contrast to modern habits, that the early dinner, in the time of Augustus, appears to have been a mark of Roman luxury and fashion.

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the relief of restless nights—to the general restoration of the body when weakened by illness, and to the removal of the depression and distress from which many suffer early in the morning,—by a small quantity of light food taken during the night; the time, kind, and quantity of such food being varied according to the circumstances of the case. The benefits of this simple practice are too little understood by physicians themselves; and accordingly it is little in use in this country, though well fitted to supersede many more ambiguous means directed to the same objects.[1]

The reciprocal importance of sound and sufficient sleep to a healthy state of the digestive organs, is familiar to all. This influence is probably in great degree of indirect kind; in some respects certainly so. There is a circle of relations here, which, even if it were expedient, could scarcely be viewed separately. The connections are those upon which all right practice is founded, and the knowledge of their causes is that which gives medicine its character as a science.

Treating the subject thus cursorily, I may omit all those details as to particular articles of food which are to be found in most works on diet and digestion. The number of such facts has been materially increased of late; and more certainty and value given to them, both by direct experiment and by their association with organic chemistry. Notwithstanding this, fashion still too largely tampers with the whole subject of dietetics; and injuriously, as regards the stability of its principles, and their application to practice. Of late years, for example, this fashion has directed itself against vegetable food;—an erroneous prejudice in many, perhaps in the majority of cases. Allowing, what is partly proved, that vegetable matters are carried indigested to a lower part of the alimentary canal than animal food, and admitting that more flatulence is usually produced from them, it still is the fact that a feeble digestion suffers no less, though it may be in different ways, from an exclusively animal diet. Morbid products are alike evolved; and some of these affecting not only the

[1] This remark will apply to other cases more serious than dyspepsia. The morning perspirations of hectic fever, for example, may often best be relieved by a little wine and water, with a few mouthfuls of light food, taken about the time of their coming on.

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alimentary canal, but disturbing other organs and functions through changes produced in the blood.

I know the case of a gentleman, having the calculous diathesis strongly marked, in whom animal food taken for three or four days, even in moderate quantity, invariably brings on discharge of lithic acid, as sand or gravel; suspended upon return to vegetable diet. This is a particular instance; but experience in gouty cases furnishes frequent and striking notices of the same general fact; thus indicating a large class of disorders, having much kindred with dyspepsia, in which excess in animal food rapidly becomes a source of mischief, not merely by overloading the alimentary canal, but by introducing morbid matters into the system at large. A steady abstinence from such excess may be counted among the most effectual preventives of gout in all its forms.

A common and in its effects, often a serious error, is that of regarding animal food as the main source of bodily strength and support, and especially such as is taken in a solid form. Without dwelling on the important and somewhat unexpected facts recently disclosed by chemistry, that vegetable matters used for food, do actually contain in different forms and proportions, the three great constituents of animal food,—albumen, fibrine, and caseine,—it is enough to say that the distinction just noted is not founded on fact.[1] The flesh meat upon our tables is itself created out of vegetable substances; and though we may admit that it contains a larger proportion of nutritive matter to its weight than any other kind of food, yet is this very quality often a hindrance to easy and perfect digestion. Exuberance of nutriment, as of any of the other goods of life, is frequently rather a curse than a boon to the body.

The rule of health being obviously that of blending the two kinds of food, I believe the exception more frequently required to be that of limiting the animal part in proportion to the other. The fashion of the day sets it down otherwise; and this is one of the subjects where loose or partial opinions easily get the force

[1] It is worthy of note that in wheat flour all these three nitrogenized products of vegetation exist. Their combination with sulphur in definite proportions, in the vegetable as well as animal form, is another of those wonderful relations which organic chemistry has disclosed; promising access to still deeper mysteries in the history of organic life.

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of precepts with the world at large. Though it is generally difficult to persuade a dyspeptic patient to such change, yet have I often attained much good by accomplishing the object, and this even in cases where flatulence, acidity, or diarrhœa were among the symptoms of disorder habitually present and requiring relief.

The singular and seemingly wayward appetites which are frequent in illness or during convalescence, and the gratification of which is sometimes even a remedy in our hands, give proof how much we have yet to learn as to the various states of the stomach, and their relation to different kinds of food. However difficult to apprehend, it is clear that this organ, together with the parts associated in the function, is often an exponent for the rest of the system of other wants beyond those of mere quantity of aliment. And upon this remarkable instinct in the animal economy (for it is difficult otherwise to express it) depends, doubtless, the preservation of that balance among the constituents of the blood which is essential to the healthy state. In all diseases it is probable that this balance is in certain degree altered or impaired; in some certainly much more than in others. The anomalous appetites of illness or convalescence are often, it may be presumed, natural efforts to restore it. There is enough of authority for this view from observed facts, to justify the physician in keeping constant watch upon such suggestions, however fanciful in kind. Separated by due caution from certain obvious sources of error, they become frequent and valuable aids in the treatment of disease.

Another fashion, as it may be termed, among rules for diet, has been that of limiting the more solid part of the food to one or two articles, under the idea that digestion is better performed upon simples than on food variously compounded. In dyspeptic complaints, or with any active disorder present, there is general truth in the maxim. But even here limitation of quantity is more important than that of quality; and care is ever needful not to oppose our partial knowledge to the natural appetites just mentioned, when well attested to be really such. In ordinary health the habits of life rarely allow the rule to be persisted in, and the conditional objections to it are, on this account, of little moment; otherwise it might be urged that extreme simplicity in the kind of food is not required by any proof we have

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of its effects on health; and is even contradicted to a certain extent by the best experiments we possess on the subject. The researches of Dr. Prout, founded on a simple and just principle, have rendered more exact than heretofore our knowledge of the elements of nutritious matter; and in referring them to three separate classes, with different chemical properties of each, have enabled us to understand the fitness of combinations of food, and even of some of the more artificial refinements which luxury has devised. It is obvious that there may be excess from a misproportion of these several elements; but it is equally true in principle, that there may be fault on the side of limitation and simplicity; and this inference is justified by various experiments on animals, attended with very remarkable results.[1]

The various questions regarding wine as a part of diet have always had much interest; and reasonably so, from their importance to the welfare both of individuals and communities. Here, also, much has been written, and many maxims and fashions are current, but without inferences precise enough to afford a rule for those who seriously seek it. It is difficult, indeed, to obtain this rule, where the results are so far complicated by individual temperament, by the kinds and qualities of wine, and by the various manner of its use. That it is powerful as a medicinal agent is certain; that its habitual use, within fixed limits, is beneficial to some constitutions is equally so. But we have not less assurance that in numerous other cases it is habitually injurious, in relation both to the digestive organs and to the functions of the brain. And it may be affirmed generally (as a matter wholly apart from the enormous abuse of spirits among the lower orders) that the use of wine is far too large for any real necessity or utility in the classes which consume it in this country. Modern custom has abridged the excess; but much remains to be done before the habit is brought down to a salutary level; and medical practice is, in general, greatly too indulgent on this point to the weakness of those with whom it deals.

It has been a maxim anciently, as now, that habitual excess

[1] The most striking of these experiments are derived from M. Magendie, to whom we owe much in this part of physiology.

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in liquors is less injurious than that of food.[1] And, with regard to wine, this may, perchance, be true. But we are hardly entitled thus to let off one evil by comparison with another, where each is in itself so great; and where they so often concur in fostering one common usage of excess, injurious to all the functions of life.

As respects quantity, kind, and admixture of wine or other liquors, general rules of ready practical application are the best which can be given. Those which aim at being more precise involve such various conditions, that evasion on the part of the patient is easy, and sure to follow. Here, indeed, it is essential to engage his good faith and understanding in concurrence with the precept imposed, as the only protection against the facility of indulgence everywhere at hand. Even under the eye of the physician, appeal must be made more or less to his own observation of effects; and this observation, therefore, it should be an object of all rules to cultivate and direct with as little ambiguity as possible.

Much may be done in this way. As regards quantity of wine, for instance, the tests of what is to be deemed excess lie within the scope of ready remark, provided attention be fairly directed to the repetition of similar effects under circumstances reasonably alike. If the excitement of the spirits exceeds that of simple comfort (a condition not difficult to note), then is it certain that there is a state of brain, the frequent recurrence of which becomes a source of serious mischief both to body and mind. Or if, as happens in other constitutions, heaviness and drowsiness ensue speedily on the wine taken, equally is it certain that the quantity is in excess, and will be injurious in proportion to the frequency of repetition.[2] Or if a hot dry skin, and increasing thirst, the inference is the same, and the result no less assured. Or, again, if the early hours of the morning are languid and oppressed, with headache of one kind or another, foul mouth, and weak or

[1] "Sæpe, si qua intemperantia subest, tutior est in potione quàm in escâ."—*Celsus.* Many other remarks on this subject might be quoted from the ancient physicians, remarkable for the truth of their application, notwithstanding the great diversity both as to the materials and preparation of food, and the habits of life most pertaining to digestion.

[2] As regards the two opposite effects here noted, I believe it will be found that the latter is more common where the kidneys act but little under the influence of wine: a result, if correctly stated, which it may not be difficult to explain.

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disordered stomach, the "*hesterna vitia*" may fairly be called to account, and wine probably as principal among these.

The familiarity of such tests makes them more valuable for practical enforcement; and there are few cases where the patient may not obtain from them, if fairly dealt with, a sufficient rule for his guidance. The same tests will apply to kind of wine as to quantity; but more ambiguously, from the many new conditions brought in, which neither chemical examination, nor common experience, have yet been able to explain. The importance of the question is fortunately much less than in the other case. It is true here, as with regard to solid food, that, if the quantity be duly limited, the kind, though by no means to be disregarded, becomes of much less significance. It may even be doubted whether any practical good has hitherto been gained by our researches into and classification of wines. The comparison of qualities, of the influence of which upon the body we are very slightly informed, gives a specious license to the indulgence in certain kinds, often quite as hurtful as the more careless use of which the preference of the moment forms the only rule.

We may admit, then, the simple tests just stated as the best that can be had, either for medical precept, or the self-direction of the patient. And, if strictly attended to, they would probably suffice for every purpose in practice. It may be added further that it is the part of every wise man, once at least in life, to make trial of the effects of leaving off wine altogether; and this even without the suggestion of actual malady. The point is one of interest enough in the economy of health to call for such experiment; and the results can seldom be so wholly negative as to render it a fruitless one. To obtain them fairly, however, the abandonment must be complete for a time: a measure of no risk even where the change is greatest; and illustrating, moreover, other points of temperament and particular function, which it is important to every man to know, for the right guidance of his habits of life.

In the common treatment of dyspeptic disorders in this country, it cannot be denied that injury is inflicted by the use of purgatives to excess. I shall speak of this elsewhere; but the frequency and ill effects of the abuse warrant every caution on the subject. It may in truth be affirmed that the complaint is often brought on by the prevalence of such treatment in

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modern practice. The imagination of the dyspeptic solicits the momentary relief thus obtained, at the expense of his future health and comfort. Scarcely a symptom of the disorder which is not aggravated by the habit of frequent purging; either directly from irritation of the alimentary canal, and disturbance of the natural course of its functions; or indirectly, by morbid influence on the mind of the patient, an effect not less noxious than the former. The general course of English practice is doubtless still open to much reform on this point.

A point of consequence, also, is the manner of using bitters in the treatment of stomach disorders. Experience leads me to think that this is generally too large and indiscriminate. Even were our knowledge of their relative qualities more exact than it is, and our adaptation of them more just in consequence, we still might often render this action more beneficial in these complaints by lighter preparations than are usually employed. Bark, as having other and specific objects, may be excepted in part from the question. But there are various states of stomach in which the ordinary doses and strength of bitter infusions are injurious; while obvious good is got from a more moderate employment of the same means.

Frequently the best mode of using bitters in these cases is in direct combination with the aperient which may be necessary. Thereby a smaller quantity of the latter is usually rendered effectual; and the noxious effects of repetition materially abated. The addition of creasote to laxative medicines is beneficial in similar way; and for this, as well as other reasons, it deserves more trial in dyspeptic cases than it has yet obtained.

The theory and uses of tonic medicines—exceedingly vague, it must be owned, in every part of practice—are not least so in their relation to disorders of digestion. Of those fitly thus termed, iron, in one or other of its preparations, is, according to my experience, the most generally beneficial. Comparing it with the whole class of bitters, I doubt whether there is any one among the latter capable of being made so variously useful. Regard of course must be had, in employing it, to the cause and particular character of the dyspeptic symptoms. But the circumstances which create apprehension (often most needlessly) as to its use, may be readily obviated by its conjunction with whatever laxatives are required; and this, as in the instance of

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bitters, is often better done by direct combination than by separate employment. The ammoniated iron with an aloetic medicine,—or the sulphate of iron in solution with the sulphate of magnesia or soda—will be found among the most beneficial of these forms; and applicable in numerous cases, without other difficulty than what may arise from the patient himself. For the imagination of the dyspeptic, as before mentioned, is ever awake to discover sensations and draw inferences; the latter often as much disordered as the feelings which prompt them. His prejudices, easily excited against any remedies, obtain a sort of countenance, in the case of steel medicines, from the phrases vaguely applied to them by the ignorant; and often needs discretion and firmness in the practitioner to insure that persistence in their use which alone can render them of avail.

Though not seeing cause for the apprehension as to alkaline medicines which some have entertained, I believe that they are often employed in needless, perhaps injurious, quantity, and indiscreetly as to the time of taking them. As they cannot fitly be termed a remedy for dyspepsia, but a relief merely to one symptom of the disorder, there is reason for closely conjoining their use with that part of digestion which it is the object to alter. Taken together with the food requiring this correction, the effect is generally more beneficial than when used after acid has been actually formed. It is a manner of prevention better according with the whole function of digestion, and avoiding that sudden extrication of fixed air, which, if the alkaline carbonates are employed, is sometimes injurious to it. The larger dilution, too, thus given to the remedy, probably aids and equalizes its effect.[1]

[1] The suspicion that the alkalies, taken medicinally, may produce a morbid state of the blood, and thereby disease in different organs, though it cannot be affirmed impossible, under excess of use, yet is not warranted by any ordinary experience on the subject. The effects of sudden injection of carbonate of soda into the circulation cannot be received as sufficient evidence. At the same time, the singular importance of this great relation of acid and alkali in every part of the chemistry of animal life, and the unquestionable agencies of alkalies on certain secretions, presumable through the blood, fully prove that the habitual use of this remedy is not negative in effect; and we have probably yet to learn some of its effects, only partially shown when taken to neutralize acid in the upper part of the alimentary canal. The Liquor Potassæ, sufficiently diluted (a point of some consequence to its successful employment), is perhaps the most beneficial form when a constitutional effect is desired.

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Celsus mentions the "*clara lectio*" among remedies for dyspeptic complaints; and this merits more attention than it receives.[1] As the condition of the abdominal viscera affects the organs of respiration and the voice, so reciprocally does the free exercise of the lungs and diaphragm influence beneficially the organs of digestion; and this effect may even perhaps be stated as one of the causes which make it more salutary to eat with others than alone. The effect is doubtless in part one of mechanical kind. But, since in dyspepsia the arterialization of the blood by the lungs is generally deficient, it may readily be conceived that certain sustained and equable efforts of the voice are further beneficial in exercising the respiration, and thereby remedying the default.

The influence of free air and sufficient respiration is, in fact, a point of singular importance in the treatment of this disorder, even independently of those exercises of the body which are usually and fitly recommended to dyspeptic patients. Good air is essential to perfect digestion: close and crowded rooms evidently disturb and impair it. The experience of every one may afford proof of this; yet is it certainly not enough regarded in common practice.[2] The dyspeptic, distempered in his feelings, languid in muscular power, and with feeble circulation, willingly indulges his indolence by making drugs the sole remedy for his ills; and this preference is too much sanctioned by the ordinary course of medical treatment in such cases.

The habits thus generated are the reverse of those which lead to cure. Neglecting or avoiding fresh air, the patient forfeits the safest and most effectual of all remedies; one which goes furthest towards the root of his disorder. For, in dyspepsia, no symptom is better marked than the languid circulation of the blood through the extreme vessels. This is especially obvious in the state of the skin and extremities of the body; and, looked to as a habit, forms one of the most essential characters of the disorder. If we might suppose, what is probable from the corre-

[1] Si quis stomacho laborat, legere clarè debet—post lectionem ambulare, &c.

[2] A familiar illustration may be drawn from the singular difference of effect in travelling on the outside or inside of a carriage, immediately after a meal. Though the food be alike in both cases, digestion in the first goes on easily and healthily;—in the second, often with much disturbance and difficulty, increased in proportion as the air is more confined.

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sponding feebleness of the heart's action, that the same condition of faulty circulation extends to the capillaries of internal surfaces and secreting organs, we obtain a general expression of altered state and balance of blood throughout the system, which might well admit of being received as a proximate cause, and as rendering explanation of many of the multiform symptoms of the complaint. The absence or diminished quantity of blood in the capillary system, through which all the more important functions are performed, and its stagnation in the great vessels, and particularly within the venous system throughout the body, illustrate in especial manner some of the more singular anomalies of the disorder.

Here the better arterialization of the blood is a main remedy; and without which none other can be of thorough avail. Open air and free respiration are to be sought for; under some condition of bodily exertion, if possible; but without it, if this be prevented. In another place I have mentioned the benefits to be obtained from the direct exercise of respiration; and these are scarcely less obvious and assured in dyspeptic complaints than in others which seem more expressly adapted to them. All that can give air free ingress to the lungs and to the blood, if not a primary remedy, is, at all events, so powerful an accessory to cure, that it behooves the physician to keep it ever before him in his treatment of the disorder.

These considerations involve in some part the theory of dyspepsia; a topic, however, which I refrain from here, as implying a more exact definition and history of the complaint than is intended in these general remarks. The ordinary states, bearing this name, are produced chiefly by circumstances in diet and mode of life acting especially upon the organs of digestion. But that one remarkable form of it, connected with hypochondriasis, and often inducing atrophy, depends on a state of the nervous power, is likely, or perhaps even certain.[1] What this state actually is, whether of deficiency or depravation, or what part of the nervous system is chiefly concerned in it, our present knowledge does not enable us to affirm. These are points involving

[1] In the writings of Dr. Whytt, so fertile throughout in sound and original views, will be found some excellent remarks on atrophy from morbid state of the nerves of the stomach and alimentary canal.

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many curious relations in pathology, and upon which we have much still to learn.

An example illustrating the connection between the states of dyspepsia and hypochondriasis, and not less remarkable in itself, is the effect of nursing in certain female habits, or of any great excess of nursing in all. The experience of most practitioners will furnish them with instances where this cause has brought on extreme disorder of the nervous system; sometimes verging on maniacal state; more frequently shown in general debility and hypochondriacal depression; and in the latter case generally connected with obstinate dyspeptic symptoms. I have known cases where effects, manifestly having this origin, have continued in certain degree even for years. The more rapid mischief occurring in some constitutions is familiar to all.

Nor can we even assert the affection of the nerves to be itself a primary cause in these and other instances. It may be that the first condition is that of an altered state of blood, or in some cases a morbid matter, like that of gout, present in the circulation; and thence producing its effects, more or less directly, on other parts of the system. And there is some argument for the latter view in the undoubted connection between dyspeptic disorders and the irregular forms of the gouty constitution; a connection sufficiently close and familiar to observation to justify the belief of relation to some common cause; acting under different modifications from age, sex, and other temperament of body; as well as from variations, it may be, in the quality or proportion of the morbid matter itself. Though much here is still speculative, yet we know enough to see that future discovery lies before us in this direction.

Recurring to the feeble state of the capillary circulation in dyspepsia, whether this depend on altered state of the nervous power or not, equally is it important to obviate what inflicts evil on every function of life. For this object, exercises on foot and horseback, friction, and bathing in different forms, together with change of air or scene, may rightly find place before most of the internal remedies which are usual in such cases. And our treatment of the disorder would be one of greater success, were these points more steadily insisted upon, and less concession made to the morbid preference for medicines; arising partly from the natural indolence of the complaint, partly from the fashions ever

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prevailing in respect to it. This is one of the instances where the general course of practice might be greatly altered and amended. Much is got, if the dyspeptic can be rescued from means, for the most part of doubtful or injurious effect;—still more is gained if he can be led into habits which invigorate the circulation with safety to every part, and may be modified under more certain rule than any remedies of internal use.[1] Here, unhappily, as in so many other cases, the simplicity of the means forms a hinderance to their sufficient application. What is obvious to the understanding or senses can rarely be brought into successful competition with what is vague or secret in the treatment of disease.

The recommendation of travel to dyspeptic patients is familiar and reasonable. Some caution, however, is needed as to details; nor ought these to be considered below the notice of the physician. The dyspeptic cannot wisely be sent to travel alone. His sensations and habits govern him too strongly to admit of his being subjected to these without aid. His earlier way must be smoothed to him; and some concessions be made to his indolence and extravagant demands for comfort, for the better chance of surmounting them in the end. I have known many patients return from the experiment, jaded and dispirited, with whom a more judicious plan would have prevented this disappointment.

[1] The ancients, as is well known, employed friction under a more general conception of its use than we do; not merely as a remedy for feebleness or swelling of limbs, but as altering the condition of the whole body. And, without adopting all they have affirmed on this subject, it is certain that its employment, even in our climate, might be beneficially extended in a variety of cases; in none more than those of enfeebled or disordered digestion. So applied, it affords another example of the many important relations subsisting between the external and internal surfaces of the body.

Of the use of anointing, as practised by the ancients, we know from experience much less. Some explanation of its employment in hot climates may be found in the protection it gives against the direct effects of solar heat on the skin. There is reason, as well as scope, for making larger trial of it as a curative means, even in disorders of the alimentary canal. The harsh dry skin of the dyspeptic patient might be improved in its texture and functions by rubbing with warm oils, &c., where wholly unaffected by internal remedies given for the same object. I have not unfrequently in these, as well as in certain other gastric disorders, had recourse to frictions of the abdomen with simple lard, persevered in for a considerable period. In several instances I have had cause to infer that much benefit was obtained by this plan; but, like most other external means, it requires more time and perseverance than the temper of the dyspeptic will often concede to such remedies.

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It is well, in the outset, to turn the line of travel away from the tumult of cities and spectacles. The dyspeptic cannot thus violently be removed from himself; and the failure of a first attempt renders others more difficult. Open or mountain air;—varied scenery, yet avoiding any such frequent change as may too greatly excite or fatigue the attention;—occasional pauses in travelling, to obviate any heat or excitement that may exist, or to prevent such;—the bodily exercise regular in amount, varied if possible in kind, and rarely allowed to pass into fatigue;—short passages by sea, if they can be managed with comfort;—early and regular meals and hours of rest; and due attention to the state of the bowels, by the mildest means adequate to the effect;—all these points are worthy of attention in advising the remedy of travelling to the dyspeptic. Their importance, of course, varies in different instances; but I know, from a good deal of experience in such cases, that it is never wise to disregard them, as unimportant to the result. And this is the more true, as it is peculiarly the character of the dyspeptic patient to be incapable of determining these things for himself.

Among the minor conditions to be looked to in stomach disorders is that of habitual posture. The patient should be led, as much as possible, to keep the trunk of the body upright, and thrown out freely in front. The compression of the stomach, pylorus, and duodenum, by the bending of the body forwards upon itself, is exceedingly injurious to the functions of these parts; as is proved from the sensations produced by making this movement suddenly, and with some little effort, when the stomach is full. Disturbance to digestion is immediately felt in a sense of uneasy weight, continued or increased as the act is repeated. The frequency of stomach disorders amongst those of sedentary pursuits, is doubtless owing in part to habits of posture unfavorable to digestion, especially in its earlier stages.

It may seem a trivial remark, yet is worth notice, that dyspeptics have frequently the habit of *touching or pressing upon the epigastrium;* a practice readily induced by uneasy sensation there, but bad in its effects on the complaint. Whatever the causes may be, no part of the body claims exemption so much as this from every interference. I have known the habit to such extent in one or two cases, as to form a principal cause of disorder; the symptoms ceasing when it was discontinued.

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Another aid in the treatment of these complaints, deserving more notice than it receives, is the application of some uniform support, amounting to slight pressure, around the abdomen. Such pressure may be made and regulated in various simple ways. The dyspeptic gains by it, not only in the avoidance of many uneasy sensations from distension, and in the better performance of the functions which distension impedes, including the respiration as one of these; but also by the uniform warmth and freer circulation in the superficial vessels of this part of the body; effects of no small import to the healthy state of the internal membranes, the relations of which to the functions of the skin are so numerous and unceasing. The chest, as matter of custom amongst us, is carefully watched over with reference to these effects; though it may be doubted, looking to the structure and function of the respective parts, whether such care is really more beneficial here than in the case before us. Be this as it may, we cannot question the value of such aids in stomach complaints; and are bound to consider them as enhanced by the simplicity of the means employed, whether the patient so regards them, or not.

Among the external remedies insufficiently resorted to in dyspeptic cases, bathing may especially be mentioned. The warm bath under certain conditions, attested chiefly by the state of the skin;—the shower bath, cold or tepid, in other cases;—or occasionally the salt-water hip bath, similarly varied as to temperature;—offer means both of prevention and relief which no discreet physician will neglect. The methods of the water-cure (*Hydropathy*) as of late brought into use, though faulty or injurious in their too indiscriminate application, yet afford many curious and valuable results, some of them closely applicable to the treatment of dyspeptic disorders. The mere fact of the impunity with which certain of the bolder parts of this practice are carried into effect, becomes a guarantee for its power as a remedy, when used with due discrimination as to the cases and constitutions submitted to it. We may consider the treatment, indeed as one founded on that important relation of external and internal surfaces, which is ever operating in all the phenomena of health and disease, and which serves as a basis to so many facts both in pathology and practice. It is the duty of the physician to draw from it, as from every other new system or

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suggestion, whatever reason and experience indicate as good;—rejecting the abuses which so readily accumulate round all novelties of this kind, from the influence of worldly interests, of fashion, or of that vague and restless spirit which, in chronic disorders especially, is ever craving for what is new, as if this in itself were an unequivocal good.

I may state again, what indeed will be seen on the face of the foregoing observations, that they are not to be viewed in the light of a dissertation on this copious subject; but merely as remarks drawn from my experience, on points which appear to have obtained less than their due share of attention, or which are especially recommended by their general and ready application in this part of practice. On the physiology of digestion—whether accepted, or still under controversy—I have refrained altogether from speaking, except in as far as was needful to illustrate the practical objects I had in view.

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CHAPTER IX.

ON GOUT AS A CONSTITUTIONAL DISEASE.

ARETÆUS has said of Gout—*Αιτιην δε ατρεκεα μεν ισασι μουνοι θεοι, εοικυιΐαν δε ανθρωποι*—and even now, with the lapse of nearly eighteen centuries between, it would be difficult to state our knowledge of the intimate nature of this disease in very different terms. The greater part of that which is either ascertained, or strongly to be presumed, may, I think, be comprised under the following general heads:—

1. That there is some part of bodily organization disposing to Gout, because it is an hereditary disorder.

2. That there is a *materies morbi*, whatever its nature, capable of accumulation in the system,—of change of place within the body,—and of removal from it.

3. That though identity be not hitherto proved, there is a presumable relation between the lithic acid, or its compounds, and the matter of Gout; and a connection through this with other forms of the calculous diathesis.

4. That the accumulation of this matter of the disease may be presumed to be in the blood; and its retrocession or change of place, when occurring, to be effected through the same medium.

5. That an attack of Gout, commonly so called, consists in, or tends to produce, the removal of this matter from the circulation; either by deposits in the parts affected; by the excretions; or in some other less obvious way, through the train of actions forming the paroxysm of the disorder.

6. That there is intimate relation between the condition of gouty habit, and the functions of the kidneys and liver, both in health and disease.

7. And that the same state of habit, or predisposition, which

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in some persons produces the acute attack of Gout, or slower deposits about the joints, does in others, and particularly in females, testify itself solely by disorder of internal parts, including the nervous system, as well as various secreting and excreting organs of the body.

To these heads others might be added, in relation both to the predisposing and exciting causes of the disease, but less definite than the foregoing. I am aware that several even of these points are liable to dispute; and as regards more especially the existence of a material cause for Gout, formed and circulated in the system, and eliminated from it by the gouty fit or in other ways, that there have been high authorities opposed to the belief. Nevertheless I find reason to suppose that this opinion, now entertained by many, will in the end be generally adopted. All the facts and analogies furnished by recent inquiry, some of these from unexpected sources, come in evidence of it;—and though the proofs are yet insufficient for its establishment as a physical fact, there are on the other hand no arguments to attest its being either impossible or improbable; nor any different view proposed, so well capable of solving the difficulties of the question.

In the observations which follow, I have kept this principle mainly in view; convinced that in every discussion as to the origin, nature, or treatment of Gout, the condition most essential to be maintained is, that of its being a malady of the general habit; and dependent on a *specific material agent* capable of showing itself in various ways; and of affecting, directly or indirectly, almost every part and function of the body. The progress of exact observation furnishes continually new proof as to these points: and their better determination must lead us to attach less importance than formerly to the actual fit of Gout; regarding this as one only of a series of changes taking place within the system; though perhaps the most characteristic and interesting, in its obvious effect of relieving the constitution for the time from the causes of the malady.

This distinction is in every respect of much moment to the true pathology of Gout. It solves some of its most singular anomalies; and furnishes the principle to which we may best resort in all difficulties of practice. Any future progress that is made in the knowledge of the disorder, will probably depend on

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such mode of viewing the subject, in connection with a material cause. And if so established by reasonable proof, this disease becomes the index and interpreter of many obscure and anomalous affections, which have hitherto perplexed all inquiry; as well as of numerous relations with other diseases, the thorough development of which is singularly important to medical science. The proper theory of Gout is in every sense an object worthy of the most sedulous research: and to this some aid may be given, even by what are merely new methods of arranging and considering the facts best ascertained in its history.

Looking first to the hereditary character of Gout (a topic noticed in a former chapter), it may be inquired whether this diathesis consists in the tendency to form or accumulate the matter of the disease, by secretion or retention, within the system? or whether what is transmitted is some peculiarity of texture in solid parts, and particularly in the fibrous membranes of the joints, rendering them liable to inflammation of a peculiar kind, and to occasional deposition of this morbid principle, when abounding in the body from other causes?

Though it is not easy to answer these questions unequivocally, yet the whole history of the disease makes it probable that the former opinion is nearest the truth. If the latter were correct, Gout, in its most general sense, would be simply a disposition from the peculiar texture of certain parts, to show topically, what in other cases, from difference of structure, produces no like effect. This view cannot well be admitted, in contravention of the many proofs that there is some peculiar substance generated within the system, which either by its morbid nature or morbid excess, gives cause to the active phenomena of the disease; and to other effects more anomalous and inexplicable under our present knowledge, yet not less due to the same morbid agent. Without speculating at present upon the nature of this matter, it is presumably the same, whether there be hereditary predisposition, or not;—it affects the same textures, and in similar manner, as respects both the kind of inflammation, and the nature of the deposits which occur;—its metastases and irregular effects are seemingly alike in each case.

We have no reason then to regard hereditary Gout as more than a disposition to generate a certain morbid matter within the body; in effect of certain circumstances of structure, either

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favoring its formation, or preventing that excretion of it from the system, which is essential to a healthy state. Or in other words, Gout, as an hereditary disease, may depend upon some transmitted peculiarities either in the organs of assimilation, or in those organs by which certain parts are separated from the mass of the blood. And we must further admit (which analogy allows us readily to do), that the same peculiarities may exist from other and independent causes;—in explanation of the many cases, where Gout, present in the habit, cannot authentically be traced through the parents or families on either side.[1]

This question of relation between hereditary Gout and that generated in the individual is obviously of much moment to a right theory of the disease. It connects itself closely with the important consideration already noticed, viz., that the fit of Gout in the joints is but a local declaration of a disorder of the whole habit, or more especially of the circulating fluid. And, regarded in itself, it involves merely the inquiry why these particular parts should be so prone to give an active and outward shape to the general malady of the constitution.

Under this view, as well as from other considerations, we may fairly receive into the class of gouty affections those indolent swellings and permanent thickenings of joints which are evidently constitutional in cause, and admit of little relief but by constitutional treatment. The habits in which these swellings occur, their connection with urinary derangements, the textures they affect, and the nature of the deposits taking place, all prove a similarity, if not identity, of origin. In fact, there is very little difficulty in conceiving that the same morbid material cause, which, in some cases, produces the sudden and acute attack, may, in others, act by a slow process of chronic inflammation, making its deposits as gradually in the parts affected. There is not less diversity of form in other diseases, from the same cause acting upon different temperaments or textures.

These views are the more important, inasmuch as it is certain that the greater or less tendency to deposition, whether depending on structure or not, is an essential circumstance in the

[1] Sir C. Scudamore, in his work on Gout, states that, out of 213 cases, 84 only did not admit of being referred to hereditary predisposition. The liability to error in this case is chiefly, perhaps, that of making the latter class too numerous; from the frequent difficulty of obtaining proof, even where the fact exists.

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pathology of Gout, influencing the whole course and character of the disorder. Though this tendency seems in part to depend on the frequency of the previous fits, and is perhaps augmented in degree by articular deposits already begun, there are many cases where it shows itself in the earlier occurrence of the disease; and where the gouty virus, after producing a few acute inflammatory attacks, seems to expend itself chiefly in these deposits, with comparatively little activity of any other kind.

Another point of great interest in the pathology of the disease, is the singular frequency with which its morbid actions are shifted, rapidly and without apparent reason, from one place to another. In the degree of this tendency, gouty inflammation differs remarkably from most others. Though ignorant, for the most part, why such translations take place, or how their direction is determined, we may presume that very slight causes are capable of producing them, from the readiness with which we can bring gouty inflammation into a joint by trifling external provocation; removing it from another before affected. This facility of translation cannot be due alone, or even chiefly, to the texture of the parts concerned. It is more probably connected with the peculiar nature of the morbid matter of Gout, in its relation to the general mass of blood, and to the secretions and excretions taking place within the system. We do not indeed by this supposition obtain any actual solution of the difficulty; but we connect it with the general causes which lie at the root of the disease, and to which all its variety of aspects, as well as every question of treatment, must more or less be referred.

It may be noticed here that some authors of eminence have considered the translation of Gout to depend altogether on the agency of the organic nerves; deriving argument for this from those cases where the reciprocal shifting of the disease from one joint to another, or between external and internal parts, is seemingly instantaneous as to time. Admitting, however, the singular suddenness of these changes, it is certainly never greater than we may suppose to take place through the blood; seeing the rapidity with which other changes, indisputably occurring through this medium, are effected. And as we have every reason to presume the material cause of Gout to be present in the blood, and that all ordinary transferences of it are made

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through this channel, it becomes improbable that another agency should be concerned in the cases just referred to; unless, indeed, we begin by asserting every local determination of gouty action to be due to some nervous condition of the part—an idea which is far too vague to be admitted as any real addition to our knowledge.[1]

This view of the metastasis of Gout (whether from one joint to another, or between external and internal parts), as taking place through the blood, is in truth a main consideration in the theory of the disease; giving explanation of its most anomalous changes of place, and of the symptoms of more general disorder which frequently occur in the intervals between such changes. The same view also best connects these ordinary phenomena of Gout with the more obscure constitutional forms of the disorder, where no outward appearances occur to define its character or degree. In truth, it seems impossible to conciliate the various phenomena of this disease with any other hypothesis than the one upon which I have thus dwelt.

I have often heard curious description from old martyrs to Gout, of the sort of perverse course of the disorder, as it wanders among the several joints before fixing itself upon a part; the sensation in each being generally that of sudden pain, followed by as sudden a feeling of local weakness after the translation has occurred to another part. Or I have known it thus to affect several joints within a few hours, and then recede altogether; without leaving other obvious effect than this weakness in a very singular degree. These conditions chiefly occur where the attacks have been already frequent, and many joints become the subject of the disease. It would seem that a part often affected with Gout has less power of resisting any fresh access of the disorder, and perhaps also less capacity for locally retaining it. Hence, smaller accumulations of the morbid cause testify themselves in the joints in patients long thus afflicted; but with symptoms generally less acute and distinct, and with intervals between the different attacks shorter and less regular. The deposits and

[1] The singularity of the electrical states of the body, noticed by some Continental observers to exist during attacks of acute rheumatism, could scarcely be taken in evidence here, even were the same shown to be present in Gout also. In our actual knowledge of the subject, changes in the blood may be deemed as likely to produce as to be produced by variations in this agency.

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thickening about parts frequently affected may be concerned in these modifications of the disease, as diminishing the power of healthy resistance to its morbid actions.

Connected, it may be presumed, with the same causes is the fact, that the precursory symptoms of Gout in the constitution are usually more severe and protracted in the case of the earlier attacks of the disease in the joints, and often affect organs and functions which are afterwards less prone to be disordered by this cause. Irregular actions of the heart, hypochondriacal depression, as well as the more common symptoms of dyspepsia and disordered secretions, frequently antecede by months the first appearance of Gout in the extremities; and occasionally give serious alarm even to those who look with medical eye upon these ambiguous cases. I have known instances, where disordered action of the heart (such in degree, and so long continued, as to lead to a thorough conviction of disease in this organ) has at once ceased upon the first fit of Gout, and never again returned.[1] Other cases have occurred to me, where severe attacks of hypochondriasis, of long duration, have so distinctly alternated with fits of Gout, that the occurrence of one in the year has wholly superseded the other. And I cannot doubt, from my experience in this singular malady, that its connection with the gouty diathesis is more intimate than with any other constitutional or casual cause affecting the body,—a point on which I shall speak in another chapter.

In fact, the external appearance of Gout not only explains, by suspending them, the cause of the disorders just named, but in opening, as it were, places of outward deposit for the disease, seems to render its effect on internal organs less in duration and degree. This view may appear too mechanical a one for changes, even of morbid kind, in the living system. Yet it is probably that which conforms most closely to the facts of the case, and best explains the anomalies which are so frequent in this part of pathology.

A point of equal difficulty in the history of gout, and seemingly at variance with the condition just stated, is the frequent attack of some joint with gouty pain and swelling, without any well-

[1] This is a point well deserving notice at the present time, when disordered actions of the heart are not only more closely studied by medical men, but have become the subject of much greater attention and anxiety, to patients themselves.

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marked symptoms to give warning of its approach. We have here to explain how the matter, capable by accumulation of producing the attack, should have been dormant up to the time of seizure:—and why, latent thus long, it should suddenly show itself in an acute form of disease. These questions cannot be answered explicitly on our present knowledge. But it may be asserted, that there are few, if any, cases, in which some token of approaching Gout may not be discovered upon due observation, though often marked only by irregular and fluctuating symptoms, or occurring in parts remote from those affected with gouty inflammation. And further, that the same difficulty occurs in other instances of disease; where active, or what may often be termed critical, symptoms, suddenly show themselves without obvious ailment beforehand; sometimes even with the aspect of better health than usual. Although quantity of the morbid matter is doubtless concerned in some of these effects, it is probable that they depend in many cases on changes taking place in its quality also. A certain definite degree of such change may be required, before the disorder can put itself into an active shape, or affect the external parts. There is strong presumption that both these conditions, of quantity and quality, have several or combined effect in producing the varieties of the disease.

To render any hypothesis as to the nature of Gout tolerably complete, it must be made to show some cause why the disorder should seldom occur in an obvious form before the age of thirty-five; why its appearance in the joints should be so rare among females; and why the disease should be so much limited to the wine-drinking classes? We are still far from any complete answer to these questions. The general limitation here stated as to age and sex, may partially be explained on the view before alluded to, of a peculiar texture in some of the solid parts, prone to take on what is called gouty action, when a certain quantity of the morbid matter is present in the system. We have reason, however, to suppose peculiarities beyond this; and one especially, having relation respectively to each of these cases; viz., the state of the sexual functions. That there is some connection between these and the causes and course of Gout in the system, is an old opinion, and probably a just one. Little can be presumed as to the nature of this relation; but probably it is subordinate to the causes, which more directly determine the presence and qualities

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of a morbid matter in the body. All changes, gradual or sudden, in the great functions of life, must modify more or less the production of this; and none, perhaps, more importantly than those taking place in the sexual state.[1]

We have no just reason, however, to presume that the gouty diathesis is absent from the female constitution, because not showing itself in similar or recognized shape. The presumption, as derived from the hereditary nature of the disorder and other observations, is widely different. If the element of disease can be transmitted to the child of one sex, it may reasonably be looked for in the other, however different or obscure the mode of manifestation:—and this the more certainly, from the undoubted transmission of the gouty temperament through the female to her child. It becomes then matter of curious inquiry, not yet adequately pursued, what are the particular indications of such temperament in the female sex? how far is it affected by menstruation? or by what other causes in female life? Though I cannot follow out here questions which involve so many details, I may notice, as one particular observation which has occurred to me, the great frequency of hysterical affections in the gouty habit; making it probable that they are modified at least by this cause, on whatsoever else they may primarily depend.

In answer to the question why Gout should be so much limited to the wine-drinking classes, we have to refer, first, to the hereditary nature of the disease, tending to make it permanently prevalent in any class, where circumstances have originally produced it;—and secondly, to the direct influence which habits of diet have in changing both the quantity and quality of the circulating fluids; and to the many instances where even a single excess or deviation from rule in a gouty habit, will bring on an attack in the joints. It must be admitted that this is illustration rather than explanation. Looking to the effects of various modes and articles of diet, it is impossible to conjecture regarding any one, why it should have this influence. Our knowledge of organic chemistry, improved though it be, is still unequal to the details of such a theory. We can proceed little further than to affirm, that certain kinds of diet do produce distinct changes in

[1] The statement of Hippocrates regarding the infrequency of gout in women is well known. *Γυνη ου ποδαγρικη, ην μη τα καταμηνια αυτεη εκλιπῃ.* Though correct as a general rule, particular exceptions often occur to it.

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the blood, and in the secretions thence derived;—that these changes consist either in the addition of new ingredients, or in the excess, or deficiency, of those which healthily exist in the blood;—and that they may depend either on faulty assimilation, or on altered or deficient action of the excretory organs.

I am led, by various considerations, to believe that the latter is the more frequent cause, and having greatest influence in the production of Gout. Though somewhat less obvious than the direct formation of morbid matters in the blood, it is certain that the want of due separation and removal of its excrementitious parts must have equal effect in producing a disordered state of this fluid. And while admitting that wine, as the article of diet having seemingly closest relation to Gout, may act directly by engendering the materials of the disease, I think it more probable, that its chief influence is in altering the secretion of the kidneys; the functions of which organs are obviously of the highest importance in all these phenomena.[1]

The question here respecting the production of morbid states or ingredients of the blood,—whether it be directly by faulty assimilation, or indirectly by deficient excretion,—is one deserving more attention than it has yet received. We may view this distinction in reference not to Gout only, but to other disorders where there is presumption of a material cause, gradually generated and circulating in the body, and at a certain point of accumulation or change, evolving itself in some form of active disease. The whole inquiry is rendered difficult by the complexity of the agents concerned, but it cannot be doubted that more definite results will hereafter be derived from this source.[2]

[1] In many habits it appears that the attacks of Gout have relation to the amount of animal food used in diet. I am seeing at this time a very intelligent patient, in whom even a moderate meat meal for three or four consecutive days never fails to bring on painful swelling of the joints; a fact so well assured to him by some years' experience, that he rarely allows the risk to occur, except with view to ascertain the continuance of the habit. Such exciting cause of Gout is perhaps more intelligible than the actions of wine, seeing the predominance of nitrogen as an element in the secretions which are so abundant in this disease.

[2] Amongst the diseases having kindred with Gout, there can be little hesitation in giving foremost place to acute rheumatism; including the fibrous and synovial varieties of this disorder, and what may more especially be called rheumatic fever; but excluding certain neuralgic and other affections which are often classed under this name, injuriously as respects the clearness of pathological distinction. Setting aside slighter analogies, the three great relations,—of hereditary character,—of

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The various points that have been touched upon in the theory of Gout, all conduct us to the question regarding the nature of the morbid matter, which has been thus presumed to give origin to the disease. Future observation, the *longioris ævi diligentia*," is still required for the solution of this difficulty. Yet the course of recent research, applying itself to the various conditions of the blood, and to the composition of the secretions, as well in morbid as in healthy state, offers a fairer chance than at any former time of attaining this knowledge. It is probable that the discovery, if made, will show it to be—not a matter alien to the system, and wholly morbid in kind—but rather the excess, either from superabundant formation or undue retention in the blood, of some material, a certain amount of which is compatible with, or even necessary to, the health of the body. Or this view may be modified in part, by supposing that, though generated in the body, it is so, only as an excretion needful to be removed, and hurtful in its retention or accumulation there. I have already alluded to what at present is the most plausible conjecture on the subject. Without venturing to antedate our future knowledge, by expressly defining the matter of Gout to be either lithic acid, or urea, or one of the lithic or purpuric salts, or any other highly azotized principle, it is impossible not to suppose that there is produced in the blood some animal principle having close kindred with these, and morbid either in kind or by excess; —a matter in the separation of which the kidneys are largely concerned, and the retention of which in the system is the cause of various disorders, according to the age, sex, temperament, or other peculiarities of the persons affected.

The true theory of Gout clearly lies in this direction. It is

frequent and sudden translation from the joints to internal organs,—and of the peculiar disorder of the urinary secretions,—are sufficient to show, not indeed identity of disease (for this is fully disproved by observation of the symptoms in detail), but a cause of analogous kind, in which the constitution at large is concerned, and more especially perhaps the state of the blood; and of which the local inflammation and fever, as in Gout, are but indications of excess in amount.

I have made many notes on rheumatic fever in the course of practice, but they are wholly superseded by the complete and valuable treatises on the subject which have appeared of late years, both in England and France. Dr. Macleod's Gulstonian Lectures (1837) may especially be mentioned for their excellence as a history of rheumatism. We owe to Dr. Chambers some important practical distinctions in a disease where such are singularly needed.

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here that we may look to obtain more intimate knowledge, not merely of the causes of disease in its active form, but also of its connection with other local or constitutional disorders, with which it is associated by some common morbid action, if, indeed, it be not actually the cause of them. Modern observation has led us to recognize some of these relations under the names of gouty headache, gouty ophthalmia, and gouty bronchitis. My own experience would lead me to add many cases of asthma to the number. I have so often seen this disorder prevalent in gouty families, affecting those who do not undergo the disease in the joints, and ceasing wholly or in part when the Gout appears externally, that I cannot doubt the existence of this relation. Many other similar relations remain to be determined; and not the least important, those which subsist between Gout and the system of the brain and nerves. The greater tendency to apoplexy in this habit is noticed by many of the older writers, and confirmed by general experience. Reference has already been made to hypochondriasis and hysteria; and it is probable that other disorders of the same class, still less generally viewed under this connection, will hereafter be submitted to it.

The relation of Gout to the functions and disorders of the liver, is another point of much interest in pathology,—clearly attested both in the active symptoms of the disease, and by those which are common under other forms of the gouty temperament. This, moreover, is one of the points associating it with that group of maladies bearing the vague name of dyspepsia; the close kindred of which to Gout can scarcely be disputed, though it may be made a question, under the present state of our knowledge, whether the relation is one of cause or of effect. Such ambiguity we are obliged still to admit in many matters of medical theory.

The connection of Gout with cutaneous diseases is an additional topic, yet almost unexamined; though I cannot doubt, from my own observation, that certain of these disorders occur as effects of the habit in question. I have so often seen psoriasis, for example, prevailing in gouty families—sometimes alternating with acute attacks of the disease, sometimes suspended by them, sometimes seeming to prevent them in individuals thus disposed —that it is difficult not to assign the same morbid cause to these

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results, however unintelligible its mode of action under such different forms.

While noticing these connections, probable or certain, with other aspects of disease, we must recur to the kidneys as the organs of the body, upon the disordered or deficient action of which depend those changes in the circulating fluids, which have closest relation to all the phenomena of Gout. Their functions, it is important to observe, undergo variation at successive periods of life, independently of actual disease. By such variation they serve in part to the destined changes of the body at these respective periods; this influence being attested by an altered state of the secreted fluid, both in the nature and proportion of several of its ingredients. That period which begins the decline from perfect manhood, is marked generally by an excess, if it may so be termed, of the lithic acid, which continues more or less through after life;—testifying itself with greatest safety, and often remedially, by large habitual discharges of this substance from the kidneys;—becoming a source of grave and various disease where this separation is insufficient or suddenly interrupted. Much certain discovery for the future (perhaps even as respects the causes and phenomena of fever) may be affirmed to lie in this particular path of physiology. And much more of practical caution might be drawn, even from our present knowledge, as to interference with these important functions, whether in health or in the treatment of disease.

Organic chemistry has taught us how readily the elements out of which all animal matter is formed are displaced from one combination and enter into others; and how very slight frequently are the differences indicated by analysis between substances eminently noxious to the system and those indifferent or beneficial to it. We owe further to recent experiments the explicit proof of what simple observation has partly shown before, viz., the remarkable effect upon the whole mass of blood of minute quantities of certain matters brought into the circulation, leading to the inference of analogous effects from an increased proportion of one or other of its principles accumulating or being unduly retained in the body. Applying these circumstances to the secretion from the kidneys, we have here complex chemical processes constantly going on (in some small part capable of being imitated artificially), by which changes are

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manifestly made in the total mass of blood; the partial arrest of these processes by disease inflicting speedy injury on the system; and where this has been more complete, from palsy or other cause, rapidly producing a fatal event, unless relieved, as sometimes happens, by a vicarious discharge of the same principles through other organs. In the instance of urea, which has been detected in the blood, these noxious effects are more expressly ascertained; and there is no reason to doubt the same result from the other constituents of urine; particularly those which occur in the form of the lithic acid and its compound.[1]

These circumstances, now familiar to us, do certainly not identify the material cause of Gout with any of the animal excretions just named; but they tend to concentrate our views towards them, and give a much more specific direction to future research. The assured connection of the gouty with the calculous diathesis—the chemical nature of the concretions and deposits in the former—and the evidence that these deposits often become in part a substitute for the more active forms of

[1] Modern inquiry in the great field of organic chemistry has made much advance as respects the products of the urinary organs. The able and successful researches of Dr. Prout are well known, in their relation both to theory and practice. The more recent experiments of Wöhler (who has actually formed urea by combination of cyanic acid and ammonia; and of Liebig, who has obtained allantoin from uric acid, and also directly by the decomposition of cyanogen and water) possess great interest, as the first examples of animal organic substances artificially formed; and further, as showing the intimate connection among all the chemical compounds of this class; and the facility with which, by slight causes of chemical change, they may take place of each other, even within the living body.

The relation of urea to sugar in atomical composition, and the gradual change which appears sometimes to take place from one secretion to the other, as in certain cases of diabetes, may well be reckoned among the more interesting of these facts. The researches of Dr. Bright have given new importance to all that concerns the presence and proportion of albumen in the urine. And Dr. Prout has justly remarked that, although without any express evidence on the subject, we have every cause to regard the separation by the kidneys of phosphorus from the blood, as an important condition to the health of the body; the occasional excess of this element or its several compounds in the urine showing its connection with other functions in the animal economy.

Dr. Bostock, to whom we owe much in this and other parts of physiology, has suggested an excellent tabular form, as a means of recording with greater precision and uniformity the several physical characters of urine, both in health and disease. (*Med. Chirurg. Trans.* vol. xxi, p. 25.) A method of this kind is almost essential to the successful prosecution of an inquiry in which the elements are so numerous and intricate.

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the disease—all concur in further sanctioning the same general view. If we cannot affirm, that urea, the lithic acid, or other animal compounds circulating in the blood, give cause to the phenomena of Gout, neither can we, on any sufficient grounds, deny the possibility of this. And under the most cautious reasoning we are at least entitled to assume with some confidence that these matters, secreted from the kidney, are the equivalents to gouty matters present in the system; that they have certain proportion of quantity to each other; and that upon this balance depend all the essential characters of the disease—its modifications being determined by various causes, some of them topical, some belonging to general functions implicated in the effects of this common cause.[1]

[1] I find in my notes the narrative of a case remarkably illustrating these views, the principal points in which I may briefly cite. In a young man, about twenty-two years of age, painful inflammatory swellings occurred of the feet and ankles, having every apparent character of gout, and considered as such. These attacks were two or three times repeated in the course of twelve months, during which time he was gradually becoming more feeble and reduced in health. When I first saw him the emaciation was great; a general state of oppression and febrile anxiety was present, with dyspnœa, a laboring and generally slow pulse, and much drowsiness. The urine was very copious in quantity, of color like water having soap-suds diffused through it, and without the proper urinous smell. It yielded, on evaporation, a large quantity of albuminous matter (resembling what Dr. Prout has termed *incipient albumen*), which coagulated into a gelatinous mass; yet flaky in parts, and showing in others a somewhat granular texture. In some portions of urine the proportion of this matter was nearly an ounce to the pint. There existed much thirst, and considerable craving for food, with other symptoms of diabetic character.

The progress to a fatal event was rapid. The oppression of breathing, drowsiness, and debility increased. Four days before death an epileptic seizure occurred, followed by frequent but less severe convulsive attacks, and with a state of partial stupor, passing by degrees into perfect coma, which continued to the end.

A post-mortem examination showed the kidneys to be the only organs obviously diseased. They were scarcely half the natural size; the texture semi-cartilaginous and extremely hard; and resembling in various other particulars the third form of disease of this organ so well described by Dr. Bright. In the cavity of the chest there were two pints of serum effused, but without any disease of the heart or lungs. The only peculiarity about the head was that the bones of the cranium were more compact, and the dura mater more closely adherent than usual.

Looking to the history of this malady there is every reason to believe that the disease of the kidneys was the first in order—that the swellings of the joints, called gouty, were the effect of the altered secretion of urine thereby produced—that a more advanced stage of this organic disease led to the large separation of albumen from the blood—while a yet further and final change produced the obscure inflammation of the lungs, the effusion of serum into the chest, and the cerebral symptoms, which terminated life.

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This, as far as I can judge, is the step upon which we at present stand in our approach to a more complete knowledge of Gout. We have reason, on fair grounds, to deem it almost certain that such knowledge will hereafter be attained, the course of all research tending to this result. And it is important again to observe, that there are few examples in medicine, where the right theory of a disease bears so obviously and immediately upon its treatment as in the one before us; or, if this be well established, gives it such wide and determinate application.

Without entering at large into the subject of the treatment of Gout (one that has been so much discussed by modern medical writers), I may close this chapter with a few remarks, founded on my own experience, of the use of Colchicum; the most remarkable, certainly, among the *φαρμακα μυρια*, which have at all times been applied to practice in this disease.[1]

The first questions occurring are, whether Colchicum can rightly be deemed a specific remedy in Gout? and in what sense, and under what manner of operation, it is to be considered as such? Its first and most obvious action, that moreover which originally gave it the fame of a remedy in this disease, is simply the removal of gouty pain and inflammation from a joint, or the abridgment of their duration and severity. But these facts, and this particular power, are so clear in proof, that they may be accepted as the foundation of our reasoning, and as an index to all the practical uses of the remedy. Does then this operation consist in destroying the matter of Gout, by some specific change? or, in withdrawing it from the part affected into the general circulation? or, yet further, in procuring its removal from the system through some of the excretory organs? Each of these suppositions may be possible; and collectively they seem to include all the modes in which the medicine can act; unless, indeed, we admit the improbable opinion, that its influence is upon the nervous system alone. Though our present knowledge does not carry us to certainty on the subject, there

[1] We have some curious testimonies in ancient authors as to the multiplicity of remedies employed in gout. Lucian, in his *Τραγοποδαγρα*, mentions more than fifty substances, animal, vegetable, and mineral, which were used as external applications only.

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are various presumptions, clear enough to be of important application to practice.

The first of these, and that which best justifies the term *specific*, is the fact, that the action of Colchicum is not limited to the removal of Gout from joints or other textures usually affected; but extends to the relief of the disease when present in parts differently composed, or when assuming the most irregular and changeable aspects. The proof here is wholly that of experience, and, it must be admitted, comparatively of recent date; but nevertheless sufficient to authorize the view just stated. We have not indeed much evidence applying to the acute forms of what are termed retrocedent and misplaced Gout; and in such cases, other and still more instant remedies are often required by the urgency of the symptoms. But in all chronic forms of the constitutional disorder, the influence of Colchicum is striking and well defined. We find it relieving, for example, the peculiar ophthalmia of gouty habits, where other remedies, local or general, have been of little avail. I have used it in that particular class of headaches, which I doubt not to be connected with this diathesis, and have obtained similar proof of its efficacy here. The same, though less explicitly, in gouty bronchitis. I have more recently used it in cases of hypochondriasis, where there was a special cause to suspect this peculiar habit; and with results warranting at least the repetition of the remedy in similar instances.

These and other examples clearly show that Colchicum is not merely a local remedy for the disease. Its power of removing gouty inflammation from the joints, though that which has attracted most attention, and given fame to the medicine, is subordinate to its action on the matter of Gout throughout the system; and it is to the latter that we must look for explanation of those effects which may be thus deemed specific in every just sense of the term.

Such explanation would be impossible to our present means of research, were we obliged to suppose any direct action of Colchicum upon this material cause of the disease. But the supposition is not necessary, nor is it even probable. What we seek, is more likely to be found in the action of the medicine on some organ, the function of which is expressly connected with the morbid conditions of gout. And pursuing this course of

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inquiry, we again come to the kidneys, as the organs seemingly concerned more than any other in these changes, and at the same time most readily and extensively affected in their functions by extraneous agents.

While allowing that there is much ambiguity on the subject, it will, I think, be found that the action of Colchicum upon the kidneys is better marked, and more considerable, than on any other part; and this too in cases where no gouty action is presumably present at the time. Though I have given some attention to the subject, and feel assured that the medicine has effect in altering the urinary secretion, and often rendering it more alkaline, independently altogether of disease, I have never been able in private practice to obtain results free from ambiguity, or constant enough to satisfy me of the precise nature of the change. It is obviously not one of mere quantity of fluid (though this appears to be generally increased), but involving, if the inferences be just, an alteration in the nature or proportion of the animal compounds excreted through this channel from the blood. I have already alluded to that remarkable class of urinary ingredients which, in some one or other of the various changes they undergo, have manifestly closest relation to the phenomena of Gout. The complex nature of these agents, and the facility with which such chemical changes take place, from their actions on each other, as well as on the other animal or saline ingredients of the urine, render this inquiry one of singular obscurity, and only to be pursued by experiments expressly made and repeated, so as to obtain a large average of results. I cannot doubt, however, that knowledge will hereafter be acquired, resolving these questions sufficiently to afford a principle for that which is now matter of empirical practice.[1]

The intimate connection between true gouty action and alteration or disorder in the secretion of the kidneys, is certainly an argument that Colchicum owes its virtue in the disease to a specific influence upon this secretion. It may be objected that

[1] Knowing, as we do on good authority, that the average of solid contents in the urine passed in twenty-four hours, and of the common specific gravity, amounts to nearly two ounces;—and that part, perhaps the largest proportion, are matters, the separation of which from the body is essential to health,—it is easy to conceive the influence on the system of alterations in this function, and the importance of all medicines which expressly act upon it.

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were this so, we should have some more marked testimony; seeing the extent and suddenness of the effect upon gouty swellings. But this objection, it will be seen, applies equally to all different views regarding the action of Colchicum. And further, we must recur here to the fact before noticed of the singular proneness of gouty inflammation, even from the slightest causes, to change its seat from one joint to another, or suddenly to recede within the system. A change of action induced upon the kidneys, as the immediate effect of the medicine, may be sufficient to check or withdraw the inflammation from any given part; and such result is perfectly compatible with the analogies we derive from other sources. The instances are frequent and familiar in the animal economy, where a new action begun, or an irritation suddenly given to another organ, will at once alter or remove morbid actions before existing; and the principle is one upon which we act largely in the treatment of disease.[1]

The case before us, indeed, is in no way more difficult to conceive, than that a single excess in diet should be capable of bringing the Gout into a joint; and the one fact is not only an illustration, but an argument for the other. If the use of the Colchicum be suspended at this moment of retrocession, the matter of the disease, thus drawn into the circulation, and not provided with any further or sufficient egress, may be presumed ready and apt to show itself in fresh attacks of local inflammation. If, on the other hand, the altered action of the kidneys is sustained by the continued use of the medicine, it may be capable of removing the gouty matter from the system altogether; or that surplus of it at least which becomes a source of active disease.

This general view, more probable perhaps than any other, will explain many seeming anomalies in the effect of Colchicum; and indicate, if correct, the best methods of its use. In particular it shows, why three or four large doses, which may often suffice to remove Gout from the extremities, do not so rid the body of it as to prevent recurrence after some short interval of time. The draining away of gouty matter is begun, enough to withdraw a local inflammation very easy of displacement; but the action

[1] It is worth while here to advert to the fact, that the kidney is the organ in the body which receives the artery of largest size in proportion to that of the glandular structure.

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needs to be sustained for its more complete removal from the system.

No point is so important as this to the right estimate of Colchicum as a remedy in Gout. A suspicion has existed (formerly, as well as since the recent revival of the medicine), that, though capable of relieving the present paroxysm, it renders the attacks of the disorder more frequent.[1] This, if generally true, would be a serious impeachment of its value; and might warrant, in most cases, the older method of treatment by sufferance of the local inflammation to the end. On my experience, however, I believe this opinion to be justified only where the medicine has been used imperfectly; or without those other precautions which are more or less essential to its success. I can scarcely doubt the expediency of carrying its employment beyond the mere relief to the local inflammation of the disease. The remedy, with due care, may be made preventive as well as curative of Gout; and, according to my experience, with no less safety to the patient. This is a point of great importance in practice, and which has not yet obtained the full attention due to it. The alterative action of small doses of Colchicum in preventing gouty action, if established as attainable without any countervailing ill from such manner of use, may be said to place a new method in our hands, in dealing with this malady under its many forms. The safety of the practice, under precautions readily observed, I hold to be indisputable. I have frequently given the medicine in moderate daily doses for many months together—in one instance for nearly two years—with entire exemption from Gout, and singular benefit to general health, when previously two months had rarely passed over without some attack. In these cases of long-continued use, I have often combined the Colchicum with a small quantity of quinine.

We may reasonably then, if this view be just, extend to its use as a medicine the remark before made regarding the pathology of Gout, viz., that too exclusive attention is given to the external part of the disease; and the value of the remedy in the constitutional forms of the disorder, too little regarded. Larger experience is making a gradual change in this respect; but

[1] Admitting the hermodactyl of the ancients to be the colchicum, as is reasonably supposed, we find Alexander Trallianus making this statement respecting the action of the medicine.

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there is still a tardiness and timidity in its application beyond the mere fit of Gout, which is not warranted by any ascertained risk. It has happened here, as in other instances (and especially in the case of new remedies), that the medicine has borne the ill fame of events in which it had no concern. The ordinary incidents of a gouty habit, as well as casualties from other sources, have been carried to its account, by a mode of reasoning common in such cases, and very embarrassing to the physician. Where the morbid actions are so various, and our knowledge of their causes so obscure, it is difficult rightly to discriminate the latter, or to rescue any particular agent from the charge of becoming such. Future experience may remove these difficulties, but meanwhile they press unfairly on the reputation of the remedy before us.

The faulty employment of Colchicum also, as already noticed, has doubtless added to the distrust of its expediency in Gout. Given solely to remove the local inflammation, and with scarcely a definite view beyond this, there is often great neglect of those evacuations which may be deemed most needful when the matter of the disease is thus suddenly thrown back into the system. Seldom ought Colchicum to be employed for this express object, without the combination of other means, fitted to act freely on the bowels; and Calomel, though not perhaps essential, may for the most part be preferred for this purpose. The action of the remedy thus aided, if not more rapid, is certainly more secure. In sustaining fully all the excretions (for though that of the kidneys be most important, it is not solely effective), we gain the best guarantee that the morbid matter removed from the joints is carried out from the circulation;—a due and timely attention to which point would probably remove from Colchicum the reproach it has incurred of producing more frequent attacks of the disease.

There still occurs the question, very important in practice, whether this specific remedy is rightly admissible at the very outset of a fit of Gout; or whether some scope and time should not be given to the local form of the disease, before thus arrested. I doubt whether this question can fitly be answered by any explicit or universal rule. Something must be left to be determined by particular constitution, by the frequency of previous attacks, and by the tendency the disorder may have shown to

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affect particular internal organs. There are unquestionably many cases in which I consider it well to allow some time of topical inflammation to elapse, before seeking thus to repress it; and yet more, where it is desirable to use the remedy so far cautiously, as not suddenly to effect the change. The rule here must generally be submitted to particular instances; nor would there be practical advantage in seeking to give it a more definite form.

The considerations already stated, afford cause to believe that we are still only partially informed of the value of Colchicum, or the variety of the objects it is fitted to fulfil in that peculiar temperament of which Gout in the joints is the most distinct manifestation. Its efficacy, in the more obscure and irregular forms of the disorder, must be allowed as strong proof of its specific character. And as bark, by its influence on all, associates together many intermittent affections under the presumption of a common cause, so may Colchicum furnish similar inference regarding various affections, seemingly remote in situation and symptoms.

This method of inference, hitherto very limited in its application, may hereafter be extended much further. It is one of singular value, as closely connecting the treatment with the theory of disease, and rendering them mutually corrective of each other. While thus expounding the relation to a common cause, of symptoms, differing wholly in aspect and in the organs affected, it at once enlarges and simplifies our views in every part of pathology. For the connections here are so intimate and various, that each fact well ascertained, is fruitful of results far beyond those which directly appear to the view.

I have already noticed the use of Colchicum in a class of headaches which I doubt not to be connected with the gouty habit; and my experience furnishes me with much proof of its efficacy in such cases. These, indeed, are instances where it merits a free and fair trial. Our practice has been hitherto so much at fault in certain varieties of this disorder, especially such as have a periodical character, that we are bound, for the credit of medicine, to look elsewhere than to old and inefficient means for their relief. I think it almost certain that some kinds of headache are produced by the same morbid cause in the circulation which brings on, in other persons, or at other times, true gouty affections of the joints. With due attention to the family tem-

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perament, to the individual habit, and particularly to their connection with certain states of the urine, it is for the most part easy to discriminate them; and where thus attested, Colchicum will generally be found to act as a safe and efficient remedy.[1]

The action of this medicine in rheumatic and other inflammations of the joints, is certainly more ambiguous than in Gout; though the old epithet of *Theriaca articulorum* seems to have been applied under this more general view of its use. Wherever active and beneficial in such cases—and it is certainly so in many—a similar principle of operation may perhaps be presumed; and as respects acute rheumatism, probably with some analogous relation to the morbid matter producing the disease. In cases of this kind, where no febrile action is present, the combination of Colchicum with bark will often be found very beneficial; the latter medicine obviating any injury that might arise from the continued use of the former.

It is a question of interest regarding Colchicum, what those effects are, which contradict or check its employment; and show themselves injuriously or dangerously, when the medicine is given in excess. That they are not so well defined as might be desired, is partly owing to the causes before mentioned, viz., the frequent attribution to the remedy, of symptoms which really belong to the disease; and the actual difficulty of discrimination in many cases, even to those who have greatest experience. Other cases, however, occur, where this explanation will not apply; and where there is proof that the Colchicum itself is injurious, either from idiosyncrasy in the patient, or from its own peculiar properties. Its influence in Gout attests the amount and singularity of the latter. It cannot, from analogy, be thus efficient for good, without the power of inflicting ill; and in the

[1] This relation of headache to the secretion of the kidneys, is often very strikingly marked. A remarkable case has occurred to me, of a gentleman, who from about the age of forty (which Dr. Prout has well remarked to be a common period of the commencement of lithic-acid deposits), had been subject to very frequent attacks of gravel, with constant sediments of the same nature in the urine. At the age of fifty-five, when I first saw him, this tendency had suddenly ceased; and he became affected by acute headaches, so severe as to produce urgent expressions of pain; and continuing, with scarcely the interruption of a single day, for five or six months. Symptoms of chronic bronchitis then supervened, with copious expectoration. The headaches were gradually relieved as this fresh disorder gained ground; but death ensued about two years afterwards, from a more active form of pulmonary disease. At the time when this case occurred to me, Colchicum was little known as a remedy, or there would have been explicit reason for employing it.

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nature of its alkaline ingredient, closely related to veratria, we have an explanation of its general sedative effects, as well as of the more immediate disorder of the stomach following large doses of the medicine. Though there is strong presumption that much of the specific effect of Colchicum depends on this active principle, the question still needs farther examination. Its solution might contribute greatly to render our employment of the medicine more beneficial and secure.

The proofs of injury, indeed, from wrong or excessive use, even if much more numerous than they are, ought not to affect its character as a remedy. The case is common to all other powerful agents in medicine; and further experience will teach us how to obviate these evils, or to correct any which may be inseparable from its use.

In these remarks I have treated of Colchicum generally, without reference to the several preparations now in use. Among these, I know none more certain in effect than the acetous extract, or better capable of fulfilling the peculiar purposes of the medicine.[1] There may be greater convenience sometimes in employing the wines of the root or seeds; but I am not aware of any superiority they possess, unless it be that of more rapid action, when such is especially sought for. I have generally given this extract, when continuing the use of Colchicum for a considerable time, as before described. Its combinations with calomel and other purgatives in the outset of an attack of Gout; —with morphine or other sedatives, and with occasional laxatives, in the progress of the malady;—and with alkalies and stomachic bitters in sequel to it, and for the prevention of its recurrence,—furnish us with the greater part of what is needful in this disorder, as far as medicine merely is concerned. Limiting the present remarks to one point of treatment, I do not enter on the question, discussed and disputed from an early period, regarding the use of active antiphlogistic remedies during the attack. The rules of diet and methods of life befitting the gouty habit, form another very important part of the subject; upon which, though much has been written, there yet is scope for more exact inferences, and a steadier application of these to practice.

[1] The powder of the root of Colchicum, if properly prepared and preserved, is also, I believe, a valuable form of the medicine, and deserves further trial in practice.

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CHAPTER X.

ON MORBID ACTIONS OF INTERMITTENT KIND.

Has sufficient weight been assigned in our pathological reasonings to that principle, which associates together so many facts in the history of disease, viz., the tendency in various morbid actions to distinct intermission, of longer or shorter duration, and more or less perfect in kind? Few objects of inquiry are more interesting than this, either to the physician or the physiologist. The subjection of so many diseased actions to this common law establishes relations which could not have been learned from other sources, and which have much value even in the details of practice. To the physiologist the facts are of great interest, as being effect and part of that more general principle in the animal economy by which various natural and healthy functions of the system are subject to similar intermission. In this, as in so many other instances, the clearest view of the causes and course of morbid actions, is that derived from the observation of the healthy actions, most nearly analogous in kind.

This principle of intermission, though in some of its workings it must ever have been obvious to all who observed the phenomena of life, has in later times only been distinctly recognized as such. And to modern inquiry also we owe the remark, that this intermittent or periodical character belongs especially to the animal functions; and that those of organic life show it, for the most part, in a much more obscure and indeterminate form. The most dubious cases are those in which both functions seem to be concerned in supporting one action, as in the instance of respiration; but the general distinction may be admitted as well marked and striking.[1]

[1] Bichat was one of the earliest to call attention to this principle, under the name of the "*Loi d'Intermittence*," in its application to the functions of muscular action,

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This tendency to intermission in the animal functions may justly be termed a law; inasmuch as it is natural, general, and manifestly designed. All the phenomena of mind, and all those of body which have direct relation to mind, are more or less submitted to it. The alternation of sleep and waking—a phenomenon in which so many separate functions have part, and by which all are regulated or controlled—is the instance at once most familiar and remarkable. Each organ of sense is more or less connected with this one important condition of existence; while they have besides various shorter and more irregular intermissions, depending on changes in the action upon them from without, and the proportion of this action to the excitability they possess. A closer view may lead us to associate these also with the phenomena of sleep; admitting the view as to this great function of life, that though for the most part simultaneously extending to many different organs and actions, these are not necessarily affected in the same degree, nor always at the same time. Thus considering it, one sense may be said to be partially asleep, when all the others, as well as the mental functions, are in state of perfect wakefulness. This general principle (stated at length in a chapter of my volume on Mental Physiology) is one of singular importance in the animal economy, and fertile beyond all others in the illustration it affords to the seeming anomalies which life and disease place before us. Regarded in reference to the present subject, it not only associates more distinctly all intermittent actions with the great common function of sleep, but is also important in illustrating many varieties of intermittent disorder, and indicating methods for their more successful treatment.

It may be that, in the case of the senses, the principle of intermission exists even in those elementary actions through the nerves, upon which sensation as a function depends. We have no direct evidence here, and from the nature of the subject it would not be easy to obtain such. But there are some reasons

sleep, &c. (*Anatomie Générale*, tom. i.) It cannot be doubted, however, that he has limited the law too exclusively to the animal organs, as opposed to those of organic life; in conformity with his other views of the relation of these parts in the animal economy. And the same is the case with respect to the kindred law of habit, which he has been led, by attachment to this principle of distinction, to confine too especially to the parts of animal life.

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for supposing alternate action and remission even in these subtle movements, which seem scarcely to be submitted to the laws of time and space; and it is possible that analogies drawn from other agents, now under active examination, may hereafter give more weight to the slight presumptions we possess on this subject.

The several actions and conditions of the voluntary organs furnish examples to the same effect as the senses; and interesting, moreover, from the other questions to which they lead us. Of these, one of the most remarkable is that which regards the nature of the exhaustion following muscular action; in what way, and in what proportion, the diminution of nervous power, and the diminished irritability of the muscular fibre, respectively contribute to this effect? Here, too, more distinctly than in the case of sensation, we have proof of different grades or periods of intermission;—from those instantaneous changes of action and relaxation which we know to exist in certain actions of the muscular fibres, to various longer periods, determined by causes of which we have little cognizance, but which are doubtless connected with the two conditions just noted. It seems not impossible (and there are some phenomena of morbid action which best admit the solution) that both are concerned in producing these variations of interval. Either the irritability of the fibre may be exhausted, or the nervous power which acts upon it; and the concurrence or separation of these effects must be presumed to alter variously the periods at which intermission takes place. The inquiries now suggested appertain to the general theory of muscular action; a subject on which so much research has been bestowed from the time of Haller downwards; yet scarcely perhaps with proportionate attention to that part of it in which the principle of intermission, and its many variations, are concerned.

Though all evidence is more obscure as to the involuntary muscles, yet can we not wholly omit here the question regarding the rhythmical contractions and relaxations of the muscular substance of the heart, as well as of other organs of the same class. This has been explained in certain cases as depending on the stimulus given by the fluids or other matters they convey. But the hypothesis is doubtful; and in the case of the heart it is still uncertain, after all the experiments and reasoning directed to this point, how far, and in what way, its action

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depends for continuance and uniformity on nervous power supplied to it?—from what particular part of the nervous system this power is derived?—or how it is sustained for a certain time even when the heart is removed from the body?[1] It is well worthy of remark on this subject, how frequently the irregularities of the heart's action, irrespectively of any organic disease, tend to assume a periodic character. The intermission or half-beat, or other still more minute inequalities, are often repeated at stated intervals in the pulse, showing that the disturbing cause, whatever in each case it be, accumulates and has effect at equal intervals of time.

The causes of action and intermission in the muscular part of respiration are liable to the same doubtful interpretation; augmented, indeed, by the mixed automatic and voluntary character which belongs to this great function. Passing over these, however, and other questions, which have been much discussed by physiologists, one general inference may be admitted, which it is difficult, in truth, to avoid; viz., that nervous power, or the agency upon which sensation and voluntary action in their material part depend, has the *element of quantity* in it in the most express sense—that it is literally capable of being exhausted by action, and renewed by repose. The fact is familiar in common statement and application; but not duly appreciated in all the deductions to which it leads. In every intermittent action

[1] In regard to this important question less attention has been given than they merit to those curious cases where great irregularity of the heart's action, continued sometimes for a period of years (and attributed, as I have more than once known, to assured disease of the organ), has wholly or in great part disappeared, in sequel to a paralytic seizure. I have the notes of two cases now before me; in one of which the patient, a gentleman who had suffered much during ten or twelve years from intermission or other disorder in the action of the heart, underwent an attack of hemiplegia four years ago, since which this irregularity has wholly ceased. The other instance is that of a patient who, during a period of fourteen years, possibly longer, had a pulse rarely below 120; the action of the heart so feeble as scarcely to be felt; cold extremities; and much tendency to œdema. Here a very slight paralytic attack altered the whole character of the heart's action, reducing it in frequency to seventy or eighty, with much more vigor or even irritability of pulse, and greater warmth and freedom of circulation thoughout the body, but giving at the same time an acute sensibility, not previously existing, to the least irregularity of the organ, which made the change a very equivocal benefit to the patient.

These cases have relation to the recognized effect of many cerebral disorders upon the heart; but they are more remarkable in the peculiarity of the circumstances, and in their rarer occurrence.

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more especially, whether of long or short interval, the notions of quantity, of expenditure, and re-accumulation, appear needfully to be involved, and most distinctly where the intervals approach to uniformity in time. It is difficult, if in any sense possible, to conceive a principle of intermission which does not virtually include these conceptions; and all our views regarding the phenomena become more clear in proportion as we keep them before us. It is less, indeed, an hypothesis with which we are here engaged than a necessary exposition of undeniable facts. And connecting with them the circumstance already noticed, of the variety of intervals of action even in the same function, as testified particularly in disease, we obtain a general view, widely applicable to pathology, and founded on a principle which extends to the whole economy of life.

The argument is more obvious, and apparently stronger, as applied to the voluntary organs and functions than to those of sensation; though complicated in this instance by the *vis insita* of the muscles themselves, already mentioned as modifying all these actions. Nor have we indeed any express proof upon which to refer the two functions to a single agent of nervous power. Conception fails us in seeking to assimilate effects so widely different in their nature. All we know is that they depend in each case on certain nervous structures (alike, as far as the most minute observation goes, for the sentient and motor nerves), the lesion of which impairs respectively all the functions to which they subserve.

Observation must further lead us to admit that the mind and body have some common relation to the principle just stated. Exhaustion of the one by overaction produces in most cases a like state of the other also. They are not simultaneously capable of strong and continuous efforts, such as imply at once intent thinking, acute attention to sensations, or powerful vountary action. One set of functions must give way to enable the other to remain in full exercise. The mind cannot safely or successfully be exerted while the body is under actual toil, or the fatigue consequent upon it; and for any great and continued effort of voluntary power it is needful that the intellect be in state of comparative rest.[1] Circumstances which force both into

[1] A remarkable instance of this, though it may escape attention as such, is the difficulty or impossibility of carrying on any earnest train of thought, when running quickly, or otherwise strongly exercising the body. We approach here again (as

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strong or sustained action at the same time do so at the expense of great exhaustion of the living power.

I allude to these topics, however, chiefly as they give basis to every view of intermittent actions of morbid kind. These admit of being classed in several different ways; but here, as in many other instances, no arrangement is so clear, or practically so useful, as one which connects the morbid actions with the natural and healthy functions of the same parts. The former grow gradually out of the latter; and though presenting in this morbid state many peculiar modifications, yet are there none which may not be referred, directly or indirectly, to some equivalent phenomenon of healthy action. For an outline, therefore, of the subject, the best division may be into intermittent disorders of sensation; those of voluntary organs; and those again of more mixed and indefinite kind:—each of these forming particular cases of disorder in the system, of which intermission is a marked and often a leading symptom; though the latter are so far distinct from the preceding that it is not easy to connect them by any common theory.

The phenomena of simple pain may be taken as an explicit instance under the first of these classes. Though often obscured, or rendered very unequal by the complexity of particular cases, yet the tendency to intermission, more or less distinct, will probably be found in all; and most obviously in those instances where the pain is acute, and limited chiefly to one nerve, as in the tic douloureux, toothache, and other neuralgic affections. Some varieties of headache also will occur as examples, in which the pain seems to occur by pulses or fits, with intervals tending to equality between. Where, on the other hand, extensive and complicated parts give origin to pain, and different nerves convey the sensations thence derived, the proofs become less distinct, possibly because the intermissions are separate, and do not necessarily coincide in time with each other. This consideration, however, leads us into abstruse questions regarding the functions of the nerves of sense, and their relation to the sensorium, which do not strictly pertain to the subject.

through so many different channels of inquiry) to that view which contemplates existence as a series of states inseparably blended in their sequence; but strictly speaking never in activity, or really present to the consciousness, at the same absolute moment of time.

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It is easy for any one to make observations on the intermission of simple pain, with the precaution of allowing for this complexity of parts; and with care also to distinguish between intermission, as a condition of nervous action, and the rapid alternations of more or less pain, which depend on the pulses of an inflamed part, and correspond in fact with the action of the heart. These throbbings may generally by a little attention be distinguished from the former.

The degree of intensity of pain seems also to modify the tendency to intermission, and the rate of its occurrence. This might perhaps be inferred, when admitting the notion of physical quantity into that agency, whatever it be, which ministers to sensation. Pain is in fact among the causes which tend directly to exhaust nervous power; and, when violent, often very suddenly and remarkably. The collapse which follows any acute bodily suffering, even though of short duration, is well known both to physicians and surgeons. The instances are familiar of sleep ensuing instantly upon the remission of such suffering; as in sequel to a severe operation, or in the intervals between labor pains, or on the remission of acute inflammation or spasms.

The principle thus illustrated by the action of one of the senses, derives similar illustration, and not less distinctly, from the functions of the others also. However natural the mode of excitement of any one of these, yet if it be excessive in degree, or unduly protracted, it becomes a source of morbid action; creating intermissions of sensibility, more or less complete; and which vary in uniformity or duration according to the nature or intensity of the cause. The instances of this deficient or disordered action of the different senses, testified by intermission, are equally numerous and familiar, and especially, as might be presumed, in those of sight and feeling; taking the latter term (however vague its general use) to express all sensations throughout the body, which are not included under the attributes of the other senses.[1] The function of hearing affords many similar

[1] Physiology, and pathology no less, may be said to labor in every part under the difficulties of language as applied to the subjects of internal sensations, derived from the different organs and actions of the body itself. The inquiry speedily merges into metaphysics, and is ever tending to be lost there. One portion of it, however, has been rescued from this obscurity, owing principally to the acuteness

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examples; as do more obscurely those of smell and taste. All these cases may be said to graduate from the slightest variations incidental to the healthy state, to others where for a time the sensibility is wholly destroyed. Yet all depend, I would again remark, upon a common principle, which in its healthy action is natural or even necessary to the animal frame.

Though the treatment of the disorders forming the subject of this chapter is not now in question, it is proper to advert to the many ways in which these considerations apply to practice; particularly in whatever concerns the prevention of injury or premature decay in the organs of sense. A due regard to the principle of intermission, both in health and disease, will furnish many general rules as well as particular admonitions, in aid of this important object; and more likely to be stable from having their foundation in a natural law.

The difficult questions to which I have already alluded again occur here, respecting the nature and source of the changes which constitute these intermittent actions. That they are material, and affecting the material instruments of sensation, cannot be doubted. That one modification of the agency, which circulates through the nervous system, and ministers power and action to all parts of the frame, is principally concerned in them, may with reason be presumed. It is probable, however, that some condition of structure in the organs themselves, has part in the effect, in the same manner in which the proper contractility of the muscular fibre modifies the action of the nervous power upon it. Beyond this general view we are not called upon to proceed at present; the question before us regarding only the morbid changes to which the principle of intermission is subject.

Morbid intermittent actions of spasmodic kind, in which the voluntary muscles are concerned, are more distinct and remarkable than those of sensation, and form a larger page in the history of disease. They possess great interest, moreover, from their peculiar and compound relation to the functions of mind and of organic life; the voluntary muscles being those especially subject to such disordered action; but subject to it independently of volition; and in many cases from causes of excitement with

of Dr. Thomas Brown and of Magendie;—that, namely, which indicates the relation between the simple sense of touch, and the muscular sensations invariably connected with this function.

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which the sensorium is in no way directly concerned. Epilepsy, catalepsy, chorea, hysteria, hooping-cough, and hydrophobia, furnish the most curious and characteristic instances of this class of intermittent actions; very irregular in kind and interval, and depending on a great diversity of indirect causes; but all having it in common that the voluntary muscles are affected by other agency than that of the will; and for the most part with clonic spasms, or those of alternate contraction and relaxation.

The common hiccup, familiar though the instance is, becomes for this reason one of the best illustrations of the simple intermittent spasm; occurring with regular intermission from causes and nervous actions independent of the will, but capable to a certain extent of being controlled by voluntary effort. Another example may be given in the *tremor tendinum*, or shaking palsy, where the separate spasms are slighter, and the intervals less distinct; but where the morbid action sometimes continues more or less constantly present through a series of years. And we have another remarkable instance in that quivering of the substance of muscles which is seen under certain circumstances of injury or disease, and may be occasionally noticed in the muscles of a palsied limb;—a phenomenon the more interesting, as it seems in some degree to interpret the natural action of the muscles under the will, which we have reason to believe performed by an infinitely rapid series of movements in their minute fibres.

This double capacity of the voluntary muscles, of being excited respectively by the will and by the actions of organic life, is one of those remarkable facts, or rather laws, in physiology, which link together functions seemingly the most remote; and a full understanding of which would solve some of the greatest difficulties in this branch of knowledge. While thus strikingly exemplified in the effects of certain diseases, it must be regarded in itself as strictly a natural part of the economy of life;—the subjection of the same muscular structure to two different kinds of nervous agency, to voluntary and involuntary excitement, being manifestly an important condition of health, though becoming in its excess or irregularity a symptom of disease.[1]

[1] Modern research has done much for the illustration of this interesting subject. It is now more distinctly recognized that the cerebral nerves, and those belonging appropriately to the spinal system, are separately and respectively instruments in the effects by which volition and other states of mind are related to the involuntary

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Many singular anomalies of the latter state are indeed solved, by looking to the concurrence or opposition of these two agencies directed to the same part. The more definite character of voluntary movements, and the consciousness of the will creating them, may give the belief that these are the most natural and first in order. But the fact is certainly otherwise. We may even venture to affirm, that while organic or involuntary muscular actions occur at the earliest period, of the same kind and perhaps of greater relative intensity than in adult life, voluntary power is a function of slow growth;—a nullity in infancy—unequal in its progress as applied to different muscular parts—capable of cultivation—governed by habit, sometimes almost to the suppression of the will—lessened or lost by disease. This manner of viewing it is most accordant with known facts, and will best obviate the many difficulties of the subject;—as, for example, the complex actions of respiration, the double relation of which to the will and involuntary power has furnished material for so much controversy to physiologists.

The spasmodic diseases already mentioned not only exemplify these singular relations, but furnish instances of another fact very important in general pathology, viz., the great length to which the periods of intermission occasionally extend, the series and character of morbid actions at each occurrence remaining the same. Epilepsy may be taken as a remarkable example to

actions of the body. The researches of Dr. Marshall Hall, and other physiologists, into the reflex actions of the spinal marrow, by attaching to definite portions of this nervous structure the relations of excitement and reflected motion which prevade every part of the system, have given much clearer insight into these singular connections of function: and it must be allowed that no other view explains with such precision and consistency the symptoms of the remarkable class of spasmodic diseases alluded to above. I might cite from my own recollections in practice many curious examples illustrating these topics. At this time I am attending a patient who is scarcely able to raise his right arm to his chin, in effect of a hemiplegic attack three years ago; but in whom the dressing of a seton, near the lumbar vertebræ on the same side, often twitches the arm forcibly to a much higher level. In the same patient, when yawning, the fingers of the right hand become suddenly extended; though at other times bent closely and tightly inwards, without any power of opening them by voluntary effort.

The valuable Memoir of Stromeyer (*Gottingsche gelehrte Anzeigen*, 1836), on the production of sensations by certain motions and contractions operating through the intermedium of the brain or spinal marrow, and the later researches of Dr. Laycock; furnish a remarkable appendix and confirmation to this doctrine of reflex actions.

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this effect; in which disorder the principle of intermission and repetition is often distinctly maintained, with intervals exceedingly protracted between;—tending sometimes to a regular period; though more frequently irregular, and liable to variation from causes of temporary excitement. Here, though in a new form, we must again recur to the element of quantity, to aid our conception of nervous power thus brought into action. In some forms of epilepsy particularly, it seems essential to any just theory of the symptoms to suppose accumulation, or irregular supply, of this or other agent; with the further presumption that if it be the nervous power, it is for a longer or shorter time taken out of the dominion of the will, and expended in producing spasmodic muscular movements.

Expenditure is here the fit expression, because in no other way can we adequately explain the progressive increase of irritability up to the moment of a fit, which so often occurs in the epileptic patient; and the almost entire absence of this which afterwards ensues. Such effect is more especially observed where the intervals are long, and the fits succeeding to them repeated and violent; the irritation subsiding more completely, as it would seem, in proportion to their severity. There is clearly, in these cases of lengthened periodicity, some direct relation between the time of exemption and the violence of the succeeding attacks; equally in conformity with the same general view of the cause.

In a well-marked case of tic douloureux now under my care, affecting the branches of the infra-orbital nerve, the muscles are often kept for half an hour or longer on the verge of spasm, which is eventually brought on by any sudden effort in speaking, swallowing, or even moving the tongue. If the attack is severe it removes wholly for a time this extreme susceptibility of the parts, if slight, the interval is proportionally lessened, affording a ratio of considerable exactness between the acuteness of the spasms, and the consequent degree of relief. An analogous fact has very frequently occurred to my observation in other cases of neuralgic pains, viz., that where the nights have been passed with greatest suffering, the following days have been comparatively exempt—a relation often distinct enough to engage the attention of the patient himself.

I have recently seen a case of epileptic attacks supervening

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upon apoplexy, in which there was singular proof of the accumulation of the cause of spasm at regular intervals of time. Six attacks occurred, with intervals of sixteen or eighteen minutes between; so exactly recurring, as noted by the watch, that it was impossible to suppose it a mere casualty. The convulsions appear afterwards to have been less frequent, and in a few hours they ceased altogether. I have notes of another case, where a spasmodic seizure, more of tetanic than epileptic character, occurred twice a day for many weeks successively, and almost exactly at the same hours each day. In another instance which happened to me lately, where the remarkable symptom at the time was that singular shock or jar which patients themselves often compare to an electric shock, and which, in this case, occurred with frequency and violence in the lower limbs, the attacks, when most severe in degree, came on almost exactly at equal intervals of somewhat more than two minutes; the muscles of the legs during the intermission being in a state of constant tremor, felt by the patient and visible to those around. The patient in this case described the sensation as "a sudden shot into the limbs, seeming as suddenly to disperse itself into innumerable sparks, which penetrated into every corner of them." Most persons will probably recognize in this description a frequent occurrence of ordinary health, where such jarring sensation is felt to shoot in an instant through the whole body; happening generally without obvious cause, but most frequently during the approaches of sleep. Physiology has yet assigned no cause for this curious phenomenon.

The hooping-cough, however different in its nature and causes from epilepsy, furnishes also a curious example of intermittent spasmodic actions, tending to like intervals, and these often of considerable length. When no casual irritation is present to provoke the fit, and more especially perhaps when the disorder is declining, it is remarkable how regular the times of seizure occasionally become, retaining the periodical character even to the last.

I find in my notes many other cases illustrating this subject;—one, for instance, where dyspnœa occurred intermittingly for some days together, with almost equal intervals of quiet breathing between;—the interval, as well as each period of dyspnœa, having a duration of three or four minutes;—and this singular

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intermission occurring under different postures, and even when the patient was asleep.

It must still be admitted, that there are many difficulties in the theory of these spasmodic disorders; depending in great part, as is probable, on their mixed relation to the animal and organic functions. Two distinct causes seem to be concerned, operating through different channels upon the same organs, separately sometimes, sometimes concurrently; and with every possible variety of combination in different cases. In these numerous conditions, we have large scope for explaining the actual diversities which occur to our notice, both in health and disease. In some convulsive disorders for instance, as hydrophobia and hooping-cough, it would seem that any direct influence of the sensorium is almost, if not altogether, absent. In chorea more of this influence is perceived, modifying in various ways the aspect of symptoms, which yet belong chiefly to the class of automatic actions. In hysteria, the interference is still more marked, and the circumstances become complex in proportion. Epilepsy presents altogether great difficulties, from its complication with apoplexy, paralysis, and other states of cerebral disease; and from its relation at the same time to so many other organs and functions of the body as exciting causes of its attacks. I can find no better general expression for the facts regarding this disease than that already given, viz., some abnormal distribution and action of nervous power, in which the brain and its nerves are chiefly concerned, and of which the accumulation and expenditure of this power seem to be a frequent or principal part. The effects on the circulation and respiration, as well as the convulsive motions, may all be received as subordinate effects. And even the singular phenomenon of the *aura epileptica*, in which the action seemingly begins from the extremities of nerves, is not more difficult of explanation under this view than by any other theory.[1]

[1] In that common but curious state, called *fidgets*, we find certain analogous circumstances, though of minor importance. Here there appears to be (whether depending on a state of the circulation, or more directly of the nerves themselves) an accumulation of some cause of irritation which requires muscular action for its relief. The effect so produced is, however, taken partially out of the power of the will; and testified by sudden, uncertain, or almost spasmodic movements, preceded by a sort of uneasy thrill through the limbs, which this action seems to remove for a time; but which returns with a regularity of interval often very distinctly marked.

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In these considerations on spasmodic diseases, I have not thought it necessary to dwell on the distinction usually made between the two kinds of spasm, seeing that in fact they graduate into each other. The clonic spasms best illustrate the principle of intermission; but it is frequently well marked in those of tonic kind also.

It is an important question regarding these intermittent spasmodic actions, whether there is not in certain cases mischief, or even danger, in forcibly arresting them? It would seem, as I have before mentioned, that there are some conditions of body (or we may more explicitly say of the nervous system) where the amount of the exciting cause present at the time renders this expenditure in the form of convulsive action necessary for relief. The involuntary movements are a sort of outlet or discharge for that agent which, either by excess in quantity or intensity, or by other manner of change inaccessible to research, has become a cause of undue excitement to the system, and requires to be removed from it.

These phrases may seem too mechanical, but illustrations of the reality of the fact will be found in many cases; particularly in the instance of the *vis epileptica*, already mentioned. I have never seen, but have heard on good authority, of the serious mischief produced from suddenly arresting by mechanical force the convulsive motions of chorea. One instance has been related to me of fatal event, apparently from this cause, in a young child strongly affected with the disorder. In a singular case, which has been long under my care, of spasmodic twitching of the muscles on one side the neck (very incessant, and of such violence that the head was forcibly drawn backwards and downwards, so as almost to touch the point of the left shoulder), the patient had apprehension from his feelings of any strong restraint put upon these motions, and considerable distress when it was actually attempted.[1]

Illustration from these familiar sources is frequently of more value than that furnished by rare and anomalous cases.

[1] In this case, which I attended with Sir B. Brodie, the patient was about fifty-two years of age, and had been subject for some years to irregular action of the heart, partially relieved by a severe fit of gout, but more completely removed when this spasmodic disorder came on. There was nothing to explain its original occurrence; and no other affection of the head or spine, except an occasional headache on the left side. The convulsive twitching continued for nearly a year with much severity;

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This fact, if sufficiently attested, is a curious one in the animal economy; and well deserving inquiry in relation to the healthy as well as morbid actions of the nervous system. In connection with the law of intermission in these actions, it involves the notion of quantity more explicitly and irresistibly than any other, concurring therein with all the conditions before stated as belonging to them.

It is somewhat difficult to class the several forms of intermittent headache with other affections of this kind; but those having lengthened periods of intermission may best perhaps be noted here. The equality of time often observed, even where the intervals extend to two or three weeks, or yet longer, is a very remarkable feature in these cases; and denotes a cause specific in its nature and uniform in its operation. I know instances where such intermittent headaches have occurred during the greater part of a protracted life. More frequently, however, it happens that they affect especially certain periods of life;—in this, as in many other circumstances, showing singular relation to the disordered actions of the gouty constitution, with which, as I have stated in a former chapter, I cannot doubt their close kindred and dependence on the same causes. In conformity with this view there is reason to believe that the kidneys are the excretory organ most concerned in giving relief to these cases; and principally by an increased separation of the lithic acid or its compounds. Such action may readily escape notice, where the attention is directed by the presence of pain to another part; but I infer it from close observation of many intermittent headaches, and think the remark likely to be confirmed by others.

I have thus far spoken of morbid intermittent actions, chiefly

so great in degree for some time as to make it impossible even to read from the incessant motion of the head. At the expiration of about twelve months from the greatest severity of the complaint, its symptoms had almost wholly disappeared; the treatment pursued being that of periodical small cuppings on the nape of the neck, bichloride of mercury and morphia; and, at a later time, the sulphate of copper, persisted in for a long period. The same affection recurred, however, about six months after; equally without obvious cause as at first, and scarcely less severe in its degree. In this instance it was seemingly mitigated by cupping, calomel, and a perseverance for some time in the use of colchicum, but far from being removed. I do not dwell, however, on the effect of the remedies in this case; believing these, with the exception of the cupping and the calomel, to have been very ambiguous in result; and at all events subordinate to other causes of change in the body.

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as they occur in the functions of sensation and muscular motion. There exist besides some remarkable forms of intermittent disorder, which I have noticed as a third class, having no direct or certain connection with either of the others, though not impossibly related by some common principle of morbid action to the headaches just described. The best type of these diseases, which chiefly belong to the order of fevers, is the regular intermittent fever; whether quotidian, tertian, quartan, or other variety. The cause of periodicity here has hitherto baffled all inquiry. We have no longer before us the fact of distinct intermission of nervous action; which under the notion of exhaustion and reparation of a definite power, seems at least to afford some explanation of the cases before cited. If admitting any thing like the principle of nervous intermission in fevers, or the reaccumulation of a power capable of exciting anew certain trains of action after stated intervals of time, it must, to meet the phenomena in question, be under different and much more obscure conditions than those hitherto known to us. Still, on whatsoever side we approach the subject, we can scarcely avoid the admission of some morbid matter, generated in equal or definite periods of time within the system; the phenomena of the paroxysm (as in the more distinct and special example of the exanthematous fevers) not coming on till the accumulation or maturation has reached a certain point—carrying away in their progress this active cause of disorder, whatever it be—and thereby producing the interval which ensues. The present state of our knowledge does not entitle us to go beyond these vague expressions of probability.

Though referring here to intermittent fevers, expressly so termed, yet must any general views on this subject include those also of remittent kind. The principle of remission is analogous, if not actully identical, with that of intermission; the regularity of period is often as well marked; and the two conditions not unfrequently pass into each other without obvious change of other kind between. The inquiry extends further to the old doctrine of critical days; a question of which all modern research tends to increase rather than lessen the interest and importance.

This is not the place for entering upon the theory of fever, even had I any new views to offer on this difficult topic, our imperfect knowledge of which carries difficulty and incertitude into every part of the history of disease. But in connection with

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the subject of intermittent actions, I must refer to one of the most remarkable facts in pathology; viz., the great variety of forms which simple intermittent fever assumes; not merely by changes of type, as they are termed, but still more singularly by affecting particular and limited parts of the body, with equal regularity of interval. A few of these neuralgic affections, from their greater frequency and distinctness, have been recognized by name; of which the brow-ague is a familiar instance. But I believe the cases to be much more numerous than is generally supposed, in which a determinate connection exists with this common principle of morbid action in the system. The fact is capable of being proved in several ways. First, by the regularity of the intermissions in these local affections, and by their following the various types of common ague;—again, by their succession to, or alternation with, intermittent fever in its ordinary form;—and further, by the influence of the same specific remedy in removing or relieving all such varieties of disorder. The application of these proofs to particular cases might be largely made, but it is enough to mention a few in illustration of the fact.[1]

Of the unequivocal transition from regular forms of ague to these local neuralgic affections with like periods of intermission, I have seen a singular instance in a gentleman, who, within the space of a few months, underwent tertian fever of the ordinary kind, brow-ague, and a similar affection of intermittent pain in one knee; each state manifestly suspending the other, and all relieved eventually by the free use of bark.

The cases, in fact, are numerous (though needing exact observation to free them from the chances of error), where ailments of this kind are transferred from one part to another, or alter their

[1] This long duration of the tendency to recurrence of intermittent fevers is a fact so familiar, as almost to conceal the singular conclusions it involves. I have seen numerous and remarkable examples of it in the Peninsula, in soldiers who joined the army there, after having been previously in the hospital at Walcheren. At this time I have two patients, residing in London, in whom variations of weather often reproduce distinct ague of quotidian or tertian type, the result of original seizures of this disorder when travelling in the south of Europe some years ago—or rather I ought to say, of the malaria fever of those countries, inasmuch as the more distinct intermittent type did not occur until their return to England. Other instances have occurred to me of this change in the type of disease from change of place.

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character more or less suddenly—the identity of the cause being still marked by the persistence of the same intermittent type. Such changes are more curious, and their value as medical facts greater, when they can be shown to depend on the remedies used:—as, for example, in cases I have repeatedly observed, where the brow-ague, partially relieved by quina, has testified itself by slight vertigo or oppression of the head, occurring at the same time, and of the same continuance as the previous seizure.

Among the more recent cases in my note-book, I find many of this general character, connected with the constitution left behind by the influenzas, so frequent of late years. In one family, a brother and sister, who had both undergone scarlatina and influenza in the early part of 1837, were severally affected in the course of the autumn with a severe pain in the upper jaw on one side, coming on each day precisely at the same hour, with complete intermissions, and continuing in this regular course, until removed by the sulphate of quina.[1] In another case lately, I have observed a large and painful swelling of the tonsils to appear at a stated hour every day; and so continuing until relieved by bark. Several instances occurred to me at the same time of inflammation of the tunica conjunctiva and eyelids, coming on at a given hour each afternoon for nearly a fortnight together; similarly relieved, but showing distinct tendency to return for many weeks afterwards. The recurrence of fits of coughing at one or more stated times each day was a very frequent symptom succeeding to these singular epidemics. I have recently seen a case of this kind, where a single paroxysm of coughing, like the hooping-cough, occurred at a particular hour in the morning, for two or three weeks in succession; and at no other time during the day.

Another instance is known to me, on good authority, in which the patient was affected by urgent thirst at a regular hour on alternate days, without other obvious symptom of disorder. Here also the character of the case was attested, not only by the periods of intermission, but by speedy cure under the use of the quina, when other means had failed. It is known that epistaxis

[1] In one of these cases the attack, precisely similar both as to seat and regular intermission, occurred in the following autumn, and a third time late in the autumn of the succeeding year; in each instance again removed by the same means.

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sometimes occurs with the same regularity of interval, and is obviated chiefly by the employment of the same means.[1]

Other instances occur, yet more obscure in kind, where there is seeming tendency to the recurrence of the same symptoms, in the same order, after the lapse of several days. In such cases, collapse in one degree or other may generally be noted as a symptom; thereby connecting them to a certain extent with the character of intermittent fever, but under a more anomalous form.

Examples of this kind, not always noted in their right connection, though familiar to the observant practitioner, make it certain that there is a common principle pervading these intermittent disorders of regular type; the development of which forms an important object of future research. The translation of one form and seat of malady into another, and the subjection of all to well-defined laws of time and order of occurrence (admitting, however, of certain familiar exceptions and anomalies), render this question one of the most interesting in pathology. Whether we may ever surmount the difficulties pressing upon it is yet uncertain; though the prospect of so doing is better perhaps now than at any former period in medical history.[2]

[1] Dr. Leonhard, of Muhlheim, relates a singular case of a woman in whom an attack of influenza (the severe epidemic of February and March, 1837) passed into a local quotidian intermittent, affecting first the left, afterwards the right arm, with every successive symptom of regular ague; the rigor, heat, and stage of perspiration all distinctly marked in each fit—other parts of the body wholly unaffected—the disorder cured by quinine. The most remarkable statement connected with the case is, that the pulse in the affected arm gave 90 beats, that of the other arm only 80, at the same moment of time.

We still indeed only partially recognize the many varieties which occur in intermittent fevers. Mr. Oldfield, who went up the Quorra in a steam vessel with Lander, describes with much exactness a quotidian which affected him in this river voyage; the paroxysm coming on at first about noon; then half an hour later every day, till it reached six o'clock; at this time an entire intermission of two or three days, recommencing afterwards at noon. This order was so often repeated that he could anticipate the daily time within a minute or two.

[2] The late Dr. Maculloch, in his work on remittent and intermittent diseases, has dealt with this subject more explicitly than any other writer; and in assuming a single generic disease, the product of malaria as a specific virus, has associated with it many neuralgic and other affections, sciatica, headache, tic doloureux, toothache, ophthalmia, palsy, &c. Though the generalization is obviously far too extensive, yet it marks throughout the acuteness of this author; and in connecting neuralgia generically with intermittent fever, offers something of more plausible arrangement for the curious facts noted above.

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One path of inquiry as to all intermittent disorders of febrile kind, is that which directs towards the diurnal changes in the bodily functions, as well in the healthy state as under disease;—a fact seemingly common to all animal life, but becoming more marked and characteristic in descending the scale of existence. In man the tendency of various disorders to exacerbation or remission at stated hours, affords remarkable evidence of such natural changes within the daily period, distinct from the more obvious effects produced by sleep. Even the last great change, that of death, is observed often to happen at a certain time of the day, at which on preceding days there has been a collapse approaching to it.

It is not easy indeed to obtain unequivocal evidence on this subject, seeing the great diversity as to time at which these periods of morbid action occur, even in the same specific disorder; and every interpretation is made more difficult by the disturbing influence of digestion in its several stages, and by the many other contingent habits of life equally affecting these phenomena. Nevertheless averages have been obtained sufficiently free from error, to show that the functions of respiration and circulation do undergo certain determinate changes within the day; such as can scarcely exist without influencing the period and course of morbid actions, when these are in existence in the system.[1] Such

[1] Dr. Prout has found that more carbonic acid is formed in respiration from day-break to noon, less during the remainder of the twenty-four hours. Something of periodical change in other functions must needfully be produced by this difference, on whatsoever cause it depend.

Observations on the pulse seem to show that there is an average acceleration of the motions of the heart in the morning, and retardation in the evening, independently of all causes of external influence; and greater excitability of the heart by stimulus, whether of exercise or food, at the former period than the latter. The recent researches of Dr. Knox upon this subject (*Edinburgh Med. Journal, April*, 1837) are very valuable; as are those of Dr. Guy, related in vol. vi, of *Guy's Hospital Reports*. These changes in the action of the heart, depending, it may be, on prior changes in the nervous system from sleep or other cause, have probably some effect on all functions of the body; but whether they are adequate to explain any periods of morbid action is very doubtful.

The remarkable results obtained by Peltier and Quetelet regarding the electricity of the atmosphere, to which I have had occasion elsewhere to refer, may not be without some relation to the diurnal changes in the human body, and through them to the intermission of certain actions of morbid kind. Where two maxima of positive electricity in the twenty-four hours are proved to exist, independently of the variations from transient causes, we may reasonably suppose some influence from this source, however impossible to appreciate its nature and amount.

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natural diurnal changes, inexplicable on any known principle, may depend on periodical alterations in the nervous power, of which sleep is the chief index and example. But be it so or otherwise, the main question yet occurs, whether they are sufficient to explain the tendency in certain diseases to definite periods of one or more days, and the fluctuations from one type and time to another. It must be admitted in the present state of our knowledge that they are not so. Even were a plausible connection thus established for the phenomena of the quotidian type, this alleged relation would render more difficult the theory of the others. The absence of the series of symptoms on one day becomes as difficult to explain as their presence on another; and the various subordinate types which have been described by medical authors serve rather to perplex than illustrate the question. No adequate explanation then has yet been drawn from this source; and the suggestion is merely one which seemingly leads us further than any other in an inquiry where hitherto so little has been attained.[1]

The existence of natural periods in the human constitution, more extended than the diurnal type, has been a favorite speculation with some authors; and undoubtedly the function of menstruation in all its parts furnishes justification of the doctrine, as well as foundation for the manner of reasoning upon it. When we have a periodical action, thus regular in intervals, during thirty-five years of female life, it is not perhaps a rash conjecture, that there may be other periods, connected with other functions, in which a like law of recurrence is maintained.[2] Opinions or

[1] If venturing to name any particular periods in the twenty-four hours at which changes most obviously take place, both in health and disease, I should be led, on experience, to refer to the hours of two or three in the afternoon, and the corresponding time in the night. Such statement, however, has little value, seeing its vagueness, and the numerous exceptions to it which arise on every side. The individualities of each disease produce perplexity here; and, equally so, all peculiarities of temperament and habits of life. Every variation of diet, and each particular stage of digestion, have their appropriate effect on the other functions, even on those apparently the most remote.

In adverting to the causes, whatever they be, which give an aspect of daily periodicity to some of the bodily functions, the proofs may be admitted that they have influence on the mind also; chiefly as respects the aptitude for exertion of its intellectual faculties, but partly even in relation to the character and intensity of the feelings. The physician, as well as common observer, have frequent opportunities of noticing this. The best evidence, however, is that which the cautious and candid observation of each person may furnish to himself.

[2] In other animals such natural periodical changes, both of structure and function,

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examples might be numerously quoted from different writers, in suggestion of periods of alternate days—of three days—of three months—of a year—not to mention those longer periods, which have been classed as distinct epochs or ages of life. But the evidence in most of these cases is that derived from morbid phenomena themselves; and in strict reason therefore it affords no explanation of disease from natural changes of corresponding interval or date. The acknowledgment, in fact, is needful that, with the exception of the various disorders depending upon or subordinate to menstruation, we have no authentic proof of such relations; though reason sufficient, in the curious and intricate nature of the phenomena, for diligent research through every channel that may be open to us.

The influence of certain medicines, and particularly of bark, in curing even the most anomalous varieties of these intermittent disorders, is a fact of great interest. Like the use of mercury in obscure syphilitic affections, or of colchicum in the most irregular forms of gout, it enables us to denote and class together symptoms apparently the most remote in kind, but which presumably could not be thus relieved unless depending on some common morbid cause. We have, therefore, in the specific nature of the remedy, a sort of practical test of the character of the disease; often of great importance to the consistency and success of our treatment; and related, through the principle of inquiry, to some of the most interesting questions in all pathology. This it is, as was noticed when speaking elsewhere of colchicum, which gives peculiar value to all that illustrates the action of these remedies. They are the interpreters of facts far beyond their momentary effect; and of connections between morbid states which are in no other way so definitely made known to us.

In closing these remarks on morbid intermittent actions, I may observe that they form a bare outline of a wide subject of inquiry, which well merits a much more complete investigation of all its parts.[1] The principle of intermission is one so marked

are much more frequent and definite than in man. As we descend in the scale of animal life, they become apparently more organic in kind; and are closely associated with the instincts which multiply so largely among the lower orders of creation.

[1] Since the first edition of this volume was published I have received from Dr.

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and characteristic of some of the most important functions of life, and affords such curious results in the variations of interval, even in the same function, that we may well rank it among other bases of classification for disease. And though some of the morbid actions thus designated are seemingly remote from others, as respects both periods and causes of intermission, it is probable that the tendency of further inquiry will be to give them closer association, by the discovery whence these modifications arise. Every relation to general principles is of so much practical as well as theoretical value in medicine, that there is often advantage in viewing diseases under new connections, even though less intimate than those previously adopted into use.

Henle, of Berlin, his recent work (Pathologische Untersuchungen, Berlin, 1839), which contains an interesting paper connected with the subject, entitled "Verlauf und Periodicität der Krankheit."

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CHAPTER XI.

ON THE MEDICAL TREATMENT OF OLD AGE.

THERE are some points belonging to the disorders and treatment of old age, which, though familiar to the experienced physician, are scarcely enough regarded in general practice. It is understood that the rules which apply to other periods of life require alteration here; but the causes and extent of this are not always considered. True it is that old age cannot in fitness be reckoned merely by number of years. Family temperament, individual constitution, and the incidents of life, all concur to modify the time at which those changes begin which warrant the term in a physiological sense; and affect no less the rate at which they proceed. The first of these causes—hereditary temperament—is largely concerned in them; as, for the same reason, it is in all that relates to the total duration of life. Upon this point, physiologically considered, I have spoken at some length in the second chapter of this volume. The fact itself is attested by general experience; and in no way perhaps more forcibly than by simply casting the eye over the groups of family graves in a country churchyard. They relate, as clearly as the most careful statistical tables could do, the history, in respect of the term of years, of the long and short lived families which lie below.[1]

[1] In speaking of the duration of human life, I may advert to a singular volume just published by M. Flourens, entitled, "De la Longevité Humaine, et de la Quantité de Vie sur le Globe." The reputation of the author, derived from his various researches in anatomy and physiology, might have justified the hope of a work of more exact science on the subject. M. Flourens takes it as the text of his treatise that the *normal longevity* of man is about 100 years; and that the abridgment of this term is due to the various errors and excesses, mental and bodily, which exist and abound in all the conditions of human society. His argument is based on the principle, previously adopted by Buffon and others, that the natural duration of life,

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Old age, moreover, may be said to be unequal in different individuals, as respects the different parts and functions of the body;—this diversity arising out of the same general causes which have just been assigned. Hereditary malady, or incidental disorders, often bring on premature decay in one organ, while others are comparatively untouched by time. These are matters of constant occurrence, and always important to the practitioner; but withal so complicated, as scarcely to admit of being classed or described. Experience alone can adequately teach them.

Taking the term of old age, however, in its familiar sense, there is no difficulty in recognizing certain changes that have already taken place, or are still proceeding, in all parts of the animal economy; nor doubt as to the fitness of modifying medical treatment, whether preventive or curative, with reference to these.

Bichat has defined life, "L'ensemble des fonctions qui résistent à la mort;" and the description, however quaint (and perhaps not strictly logical), is true in one important sense; viz., that living organization is that which is withdrawn for a time from what we understand as the ordinary laws and conditions of matter, and seems in many points even placed in opposition to them.[1] I use general terms here, because I believe that our

both in man and other animals, is a certain definite multiple of the period of growth of the body. But instead of estimating the growth by simple stature, as they have done, he takes the period at which the union of all the bones at their epiphyses is entirely completed, and makes this his multiplicand. He asserts, on the analogy of various animals (the horse, dog, cat, cow, camel, lion, &c.) that the proper multiple of this period is 5; and finding in man that the growth and union of bone is thoroughly completed in twenty years, he boldly adopts the conclusion already stated, "L'homme est vingt ans à croître, et il vit cinq fois vingt ans; c'est à dire, 100 ans. La durée de la vie est donnée par la durée de l'accroissement."

Though undoubtedly relations exist in animal life, in which the periods of gestation, of growth, and of duration of life, are all intimately combined, we can scarcely admit the argument and conclusion given above, without stronger proofs through which to reconcile them with the facts which every age, country, and condition of society, put before us.

[1] A grosser definition of life, though in the same sense, is given by another author,—"Illud putredini contrarium." Other definitions again take a more metaphysical shape, such as that of Gottfried Reinhold—"Leben besteht in der Gleichförmigkeit der Aktionen, bey ungleichförmigen Einwirkungen der aussen Welt." It is obviously easy to multiply definitions founded on this broad principle of description; and many in truth might be cited, which have no other merit than a similar paradoxical affirmation of some partial truth.

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knowledge does not justify our drawing any distinct line between what has been severally named physical and vital laws. Such line may exist; but the attempt to define it at present rather marks our own ignorance than any natural boundary between the laws which govern organic and inorganic creation. All great discoveries in physical science (and especially some of recent date) have had the effect of altering this presumed boundary, and, for the most part, of extending the domain of physical into that of true vital phenomena.[1] Chemistry in its largest sense,—connected on one side with the great mechanical and atomical laws which govern the universe; on the other, with those imponderable agents, light, heat, and electricity, which science is now fast submitting to the same laws,—is apparently the source whence we may expect the greatest and most certain results. Still, in all researches of this kind, a limit exists, impassable by human thought and inquiry, the right definition of which boundary is the distinctive mark of true science.

The power of vitality, however, much better defined by what it does than by what it is, cannot be regarded as one definite quantity. It fluctuates much even during the vigor of life: it declines in amount in old age; and its decay gives scope in the same ratio to those impressions upon the body of common physical agents, to which life in its entireness seems to stand in a sort of opposition. There are few more curious subjects of research than the condition of the human body, when, from the weakening of the vital power, agencies and changes, purely physical according to our understanding of the term, produce new and peculiar effects upon it; the progress of these showing the degree of decline in the power which we thus name.

Gravitation, for instance, which has manifest influence on the circulation in all parts of life, and against which many provisions are made in the structure and arrangement of bloodvessels, acts

[1] It is needful here, however (and especially for the student or general reader on such subjects), to understand the true bearing of these results, even if carried far beyond their present limits. They do in nowise alter or impair the evidence of Natural Theology, but even enforce and enlarge it. If an agency, wholly unknown to us before but in a certain series of effects, is proved to be identical with one producing, under other circumstances, a different series, we extend in our comprehension the scope of that power which can govern and direct the same agents to such vast and various ends; and we establish yet further the unity of that design which pervades, associates, and sustains the whole.

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with augmented effect as the vital moving forces are enfeebled. These changes, depending on mechanical causes, and including the various depositions and obliterations which take place in the vascular or other parts of structure, have been so fully described by different authors that it is needless even to enumerate them.[1] The further question as to changes in the blood, or the admission of chemical on the diminution of vital affinities, is a more obscure matter of research, and one upon which few certain results have been obtained. The increased proportion of venous blood in old age is one of these facts; and it is probable that some of the deposits and alterations in different textures are the result of the lessened power of life of resisting common chemical changes, especially in the minute vessels, where it is likely that such loss of power would first be felt. Whether we are to look to the same source—or more directly to the functions of the lungs—or to those of the nervous system—for the diminished faculty of producing animal heat, is a point not yet wholly determined; though belonging to one of the most interesting questions in physiology. However this may be, the importance of the fact is obvious, as relates to the medical treatment and general management of old age.

I may notice in the same cursory way the other changes, less obvious in their origin, though equally familiar to observation, which belong to this period of the life of man. The senses (often from structural changes, but often also without obvious alteration) lose their power of being excited—become less keen and discriminative. The energy of volition is enfeebled; and its influence over the muscular actions irretrievably impaired, often to the extent of partial paralysis, independently of the changes in the muscular tissues themselves. The diminution of irritability and sympathy in all textures of the body, from whatever parts of the nervous system these functions are respectively derived, seems to occur in some ratio to the decline of sensibility and voluntary power. And hence morbid changes in all the functions of absorption and secretion, on the skin without and the membranes and glands within; and the incapacity of

[1] What has been successively written on this subject by Boerhaave, Heller, and Bichat, may especially be referred to. Recent inquiries have chiefly added to our knowledge of the changes going on in the capillary vessels in old age. The observations of Dr. Carswell on the *Gangræna Senilis* are a valuable example of these researches.

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adequately repairing the injuries sustained from accident or disease.

The points just alluded to are interesting in various ways as regards the state and disorders of old age. They connect themselves with the fact—seemingly well attested, though not enough considered in practice—that at this period of life the several organs become, as it were, more insulated from each other. The relations of nervous sympathy between them are enfeebled; and in effect of this, their diseases also assume a more detached character than in earlier age. In connection with the same cause, but depending also on the want of reaction in each part from feebleness of the general functions, we find that organs may take on a condition of specific disease with comparatively little excitement to the system, or suffering in the part itself; and often, it may be affirmed, with less danger to life, from a given amount of organic change.[1] We have good reason to look upon these circumstances as a beneficent provision for abridging or mitigating the bodily ills, which in certain measure are inevitable as time "delves its parallels on the brow," and life advances towards its latter end.

The intellectual functions, and particularly the faculties of recollection and association, undergo gradual change; but not in any obvious proportion of time or degree to the decline of those powers which give us more direct connection with the external world. Where there has been no express disorder of the sensorium, some of the higher faculties of thought, as well as of moral feeling, are occasionally preserved to a very late period of existence, and amidst much of physical decay. Ultimately, however, they give way, under that provision of our nature by which the living mind has been attached in its present existence to a certain definite organization—growing with this from infancy upwards—independent in some of its faculties, as far as consciousness can inform us—in others so related to the physical conditions of structure, as to be intimately affected by

[1] These points are noticed by M. Prus in a Memoir presented to the Academy of Medicine in Paris, in 1838, in which are given some valuable tables of the relative mortality of the aged from different disorders; a subject his situation at the Bicêtre afforded him singular opportunities of studying. The most striking result in these tables is the very large proportion of deaths from diseases of the organs of respiration.

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all changes in these;—the whole a deep mystery, to which human reason in its present state can seemingly make no nearer approach.[1]

It is well worthy of note to how small a sphere of action life is often reduced, either from old age or disease, before its utter extinction; and how long in fact it may continue, thus narrowed and girt in on every side, with little excitement, and as small expenditure of those powers upon which it depends. Examples to this effect are ever before the eye of the physician. The vital

[1] The growth of the intellectual powers through infancy and childhood has been the subject of careful notice and inference. Not equally so the progressive steps of mental decay in old age: a more difficult study, from the inequalities introduced by state of health, and the habits physical and intellectual of previous life; yet still capable of yielding many curious results to the science of the human mind.

It is one opinion on this subject that the sensorial functions, later perhaps than others we possess in reaching their respective completeness in each individual, are the first which begin to decline. The assumption is a very doubtful one: but however this may be, the enfeeblement is certainly not in the same ratio as to time for all the faculties; the losses not equal of the ideas and knowledge of different classes, and different periods of acquisition. The faculty of receiving and associating fresh impressions seems for the most part to decline earlier, than the power of combining and using those formerly received. The faculty of directing and fixing combinations or successions of ideas is probably one of those earliest impaired. In a lady of 91, whom I am now attending, the decay of the mind shows itself especially in the dependence of the course of ideas on the sound of words. A word, or even part of a word, of double application, will suddenly, and without the consciousness of change, carry off the mind to a new and wholly foreign subject. In another elderly patient, whose memory is singularly tenacious as to persons and detached events of past life, there is a singular incapacity of associating them together by any reasonable link; and the slightest relations of time or place suffice to carry the mind wholly astray from its subject. Such instances, by no means uncommon, have close kindred to the state of dreaming; the power of combining and directing the course of thought being similarly impaired in both cases.

It would seem, as respects the decay of memory more especially, that the power of recollecting words and names is lost earlier and more readily than that of events, both in effect of old age, and of accident or disease. This fact is strikingly attested in paralysis and other cerebral disorders; where, the ideas still remaining distinct, words are partially or wholly wanting for their expression; or others, by a strange and inexplicable perversion, are substituted, having no relation to the intended sense.

In another place (Chapters on Mental Physiology, Chap. IV) I have alluded to what perhaps is more hypothetical, the absolute tardiness of mental operations in old age, whether of perception, volition, memory, or thought. It seems impossible to exclude the direct notion of time from these cases, as compared with others where the functions are in full vigor of power and exercise.

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organs tardily carry forward the processes essential to existence; respiration is limited to a small fraction of the lungs; the blood feebly projected by the heart moves languidly through vessels obstructed in all their minute branches; and the mental faculties, so far as they can be estimated, appear under the condition of a vague and obscure dream—an analogy capable of being extended in many ways, and of great and peculiar interest to our views of the nature of mind, but which it is not my business to pursue further here.

One observation I would yet further make on this subject, viz., that no previous reason or feeling, no judgment of vigorous health, can afford a right estimate of the relation the mind assumes to death in the latter hours of life, even where little impairment of its faculties has occurred. This is especially true when long and painful sickness has been the prelude to the event. But the exhaustion also from acute pain of short continuance alters this relation; and even without sickness or suffering of any kind, the mere diminution of vital power by general decay produces the same effect. The earnestness to live abates as the possession of life, from whatever cause, is gradually withdrawn. Every physician is witness to these things, as he watches occasionally over existence slowly ebbing away, without toil, suffering, or alarm.

"Gathered, not harshly plucked, for death mature."

Familiarity with all the circumstances which thus designate the natural progress of old age, and with the modifications they produce in disease, is obviously of much value in practice. I shall refer in what follows to a few of those points which seem especially to deserve attention.

The first practical conclusion which the prudent physician will draw from this part of his knowledge is in some sort a negative one, viz., not to interfere—or, if at all, with care and limitation—in those cases where changes, irretrievable in their nature, have occurred in any organ or function of the body. To urge medical treatment in face of distinct proof to this effect is to sacrifice at once the good faith and usefulness of the profession. This is a point the more needful to be kept in mind, as the patient himself, and those around him, are rarely able or willing to recognize it. It is often an exceedingly nice question of con-

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science, as well as of opinion, to define the extent to which practice may rightly proceed in such instances; always admitting, as must be done, that something is due to the feelings of the patient; something also to the uncertainty of our own judgments, antecedently to actual experience. This question in medical morals, like so many others, cannot be treated as a general principle only. The integrity and discretion of the practitioner must ever be appealed to for guidance in the endless variety of particular cases. In some, concession to a certain extent is safe, or even justified by indirect advantage to the patient. In others, mischief alone can arise from this meddling with the course of nature; and bad faith or bad judgment are involved in every such act of practice.

Illustrations of the latter kind may be drawn from a class of cases meriting such attention; those, namely, where discharges occur from some part of the body; the fitness of checking or suppressing which is the main question of treatment. The discredit injuriously cast upon the humoral pathology had the effect during a long period of withdrawing the attention due to this class of disorders; and which, with inferior means of research, the ancient physicians had more sedulously bestowed upon them. Modern inquiry has again reverted to the subject; and with the assurance, already sanctioned by the results, of finding in it a wide field of discovery. Whether these discharges be habitual, or critical, or vicarious (for they may be understood as occurring in each of these senses), equally do they express functions of life, which cannot too sedulously be kept before us in practice.

This important consideration applies to cases occurring at every period of life, and not least to certain symptoms common in old age. In the Catarrhus Senilis, for instance, and the various bronchial affections of old persons, we have to consider whether it is expedient to put an active check upon them (therein complying with the demand almost always made from the physician), or whether the mucous or other discharges they involve are not really a safeguard to the constitution; an outlet provided for that which, retained in the blood, would create disorders of more critical kind. Without affirming it to be so in every case, my experience assures me that in many the practice of restraining such discharges is injurious in the attempt, hazardous if it

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be really effected. I entertain no doubt, from proofs furnished at every period of life, and particularly from the evidence by metastasis (of which I have noted many curious instances), that these large mucous secretions are often as essential to health as the secretions from other organs, which we assiduously seek to sustain. The necessity for them, having doubtless some relation yet unknown to the healthy properties of the blood, varies exceedingly in different habits, and at different ages; and this variation we can often distinctly connect with the state of other secretions; sanctioning the term vicarious, though not perhaps in the absolute sense in which it is sometimes used. We may seek to moderate occasional excess; to facilitate expectoration; and to regulate the cough so that it shall interfere as little as possible with rest. But to urge practice beyond this, in control of habitual symptoms, is more frequently a mischievous interference than a right direction of medical treatment.

The latter symptom, of cough, is that for which the physician's aid is most generally invoked, and where concession is too often and indiscriminately made. For, whether the secretions from these membranes be necessary or not, the act of coughing is at all events needful for their extrication or removal; and cases must have occurred to every practitioner where it is difficult to avoid the conviction that the sudden arrest of this, by opiates or otherwise, has accelerated a fatal event. This point is always to be kept in view in treating the disorders of advanced age; as well from the frequency of the symptom, as from the mistaken views regarding it wchih are constantly pressing upon the better judgment of the physician. In another chapter I have noticed the undue apprehension which prevails as to the mechanical act of coughing, as if this were always a serious and assured mischief. In some cases it is unquestionably so; but there are others where it is even beneficial to a certain extent, in relieving stagnant circulation, and giving freer access of air to the lungs; and, under almost all circumstances, cough may more expediently be looked to in practice as an index than as a malady in itself.[1]

A similar caution may be given as to interference with the secretion of lithic acid from the kidneys, generally increased in

[1] In a work lately published by M. Reveillé-Parise, *Sur la Vieillesse*, he propounds the idea that old age begins in the lungs;—a partial and fanciful view, not sanctioned by any sufficient proof.

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amount in old age. Alarm is often excited by the large quantity of this coming away, and alkalies are as largely given to obviate it. When there is tendency to concretion or occasional lodgment in the kidneys, this may be proper; but where the discharge is free and without distress, and there is no obvious disorder of internal organs attending it, the wisest practice is, generally, that which most refrains. This discharge, indeed, is often the most effectual outlet for that which, retained and accumulated, becomes a source of active disease, particularly to the brain, in those of advanced life. I have known many patients never well, unless when such removal of animal products through the urine was largely going on.

I may further allude, under similar view, to habitual discharges from the bowels; resembling often those of active disease, yet attended with no distress, and indicating only the need of separation of these matters from the body. A meddling or rude practice in these cases is rarely effectual for good and may do serious harm if it alters or suspends actions salutary to the system.[1]

Though less frequent occurrences than some of the preceding, the same remark will apply to certain passive hemorrhages incidental to advancing age; epistaxis; hæmaturia; hemorrhages from the bowels, uterus, &c. In conformity with the oldest opinions in physic, these may often be regarded as salutary evacuations, critically occurring to the relief of particular organs, or of the general circulation. On the approach of old age, as indeed at other marked periods of life (without getting too far into the mystery of epochs), various new balances are struck in the allotment of the blood to different parts; and in the course of such changes, congestions and discharges are prone to occur; the latter relieving or preventing the former. The tests by which this is to be determined are generally simple and well marked, requiring only care to note them. And all experience teaches how frequently these discharges, even when most profuse, proceed safely to a termination: their continuance being defined by the relief they afford to the exciting causes. But here also the physician is occasionally drawn out of the temperate observation which is his best practice, by the alarm and anxiety of those who surround the patient. Without evidence

[1] See Chapter XXI. On some Points in the Pathology of the Colon.

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of inflammation or organic disease, the lancet is taken up to control, as it is presumed, the internal hemorrhage; or lead and other astringents given, with vague relation to the cause or seat of disorder, and often with mischievous effect in other ways.

Taking into fair view the average of such cases, and without any undue favor to the doctrine of critical evacuations or vicarious discharges, I believe more evil to be inflicted by eagerness to check or alter the course of hemorrhages of this kind, than can be incurred by a cautious forbearance and observation of their progress; with due care to sustain the system under any exhaustion which may attend them. Even in common cases of hemorrhage from diseased structure, this consideration is to be kept in view; but particularly where there is presumption of its taking place from mucous surfaces, or in the parenchymatous structure of organs, without actual organic disease.

I do not here dwell further on this point, because in another chapter I shall have occasion to speak more generally on the subject of hemorrhages as involving an important question of medical treatment.

The particular questions as to the relative fitness and extent of bleeding, as a remedy in advanced life, is one of frequent occurrence to the practitioner; and often involving very serious doubt. It can only be rightly solved by sound experience applied to each several case. The general rule, doubtless, is that of comparative limitation, and a still more cautious observation of the tests by which this limit is marked in actual practice. For the various disorders of inflammation or congestion occur in the same organs, and of the same kind, as in earlier life; but with diminished means in the system itself of resisting the disease, or repairing any losses sustained in its treatment.[1] Under the latter consideration, it becomes especially needful in old age (though the rule applies to all ages) that the remedy when required, should be promptly used; before the disease gains stronger hold, before parts are disorganized, or the nervous powers exhausted by long illness.

The latter point is one to which attention is singularly due at this time of life; lest by continued active treatment of symptoms,

[1] The observation of M. Louis, on inflammations occurring under debility of the system at large, well merit every attention here.

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which might have subsided after the first remedies, we bring on others more injurious in result. Here, too, there is need of a particular caution as to the pulse of old age. The artery brought nearer to the surface by the absorption of other parts, with its own texture and elasticity also changed, gives a pulse to which common interpretation will not apply; and which deceives the inexperienced or careless by the impression of more vigor of circulation than actually exists. A like deception often arises from ossification of some part of the heart or vascular system, so common at this time of life. An irritable jarring stroke is thereby produced, mistaken sometimes for one of power; and leading to a mischievous activity of depletion, the effects of which are not readily repaired.

In the application of the valuable means we now possess for discovering organic changes about the heart and great vessels through the parietes of the chest, the separate indications to the same effect through the pulse are too much neglected. It is probable that there is no structural disease of the heart, certainly none of consequence, which is not represented in some manner in the pulse, beating along a vessel of the size of the radial artery. The difficulty, of course, is in estimating the slighter modifications, and in separating them from the analogous or identical effects of other disorders; the latter difficulty, indeed, one that is common in part even to the more delicate methods just referred to.

One of the best tests in reference to the question now before us, is the *equality*, or otherwise, of the heart's action; and the more needful to be observed, seeing the frequency of structural changes in this organ in advanced age. If intermitting, and unequal either in the strength or frequency of its beats, caution as to bleeding must be doubled; and this caution is no less to be maintained, where there is presumption of the irregularity depending on sensorial disorder. In this case it expresses more frequently deficiency of nervous power, than a state of brain requiring depletion: while if originating in the vascular system, it is oftener the result of mechanical difficulty from structural causes, or of unequal supply of blood to the heart, or of change in the quality of the blood itself, than of actual excess in the quantity of this fluid. Where, nevertheless, the symptoms are deemed to require bleeding, its influence upon the regularity of the heart's

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action must be carefully looked to as a test; and this not merely at the moment of depletion, but even yet more when the circulation has recovered from the sudden change of balance thus produced.

The effect upon the sensorium and nervous power is, undoubtedly, the most important part of the question as to bleeding in old age. In the Chapter on Bleeding in Affections of the Brain, I have made some remarks which bear especially on this subject. It is certain that there are states of brain, with which its general condition in old age is closely connected, where sudden or considerable loss of blood brings on various morbid effects, paralytic or convulsive in kind, the occurrence of which is too exclusively attributed in common practice to fulness of blood in the vascular system. Errors in treatment, founded on this view, may be repaired at other periods of life, when such state of brain is brought on by casual causes: but in old age, where it is more or less permanent, and depending on organic changes, the mischief done is sometimes irreparable in kind. And the greater care and discrimination are needed here, from the singular similarity, as far as general aspect is concerned, of symptoms really arising from different causes, and requiring opposite treatment.

Further, it may be remarked that bleeding in advanced life, when manifestly required by the symptoms, does not of necessity preclude the use at the same time of means to support the nervous power. On this point, indeed, as respects all periods of life, common practice is too straitened and exclusive. A disorder which requires depletion by bleeding, as the chief and earliest remedy, is presumed to admit none other than antiphlogistic means while this state continues: and, as a general rule, this is so; but there are cases of exception, and old age includes many such. The nervous and circulating systems, though so closely connected in every function of life, have yet their separate powers. Even taking the whole of each system, these powers are not always, it would seem, in exact relation to each other; and this is more particularly true where the vascular changes, whether of inflammation or simple congestion, are limited in extent. We may need for relief the change in circulation which bleeding affords; yet may require at the same time that support or stimulus to the nervous power, which is essential to the equal distribution of the blood, and without which dis-

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orders of a new kind will supervene, or the one for which the remedy was applied be kept up under an altered form. We cannot rightly refuse the name of inflammation to certain cases which occur, not simply in conjunction with debility, but even consequently upon it; and the just application of remedies of the two classes, concurrently or consecutively, to these cases, forms one of the most delicate points in medical practice. It is sufficient to mention it here, in its application to the treatment of the diseases of advanced age.

Several of these observations as to bleeding apply equally to the use of purgatives. The frequent employment of drastic medicines of this kind is injurious to the general powers of life, as well as to the particular organs on which their impression is first made. The necessity of many cases, even in extreme age, does of course supersede this rule; but it is one fitly to be kept in mind in every part of practice. In applying it we must advert to the fact, that the feebler circulation, and deficient sensibility and secretion of the mucous surfaces in old age, often render medicines and doses ineffective which have acted readily at other times of life: so that here, even more than in other cases, we are to measure our means not by common tables of doses, but by observation of effects on the body.

Many of the foregoing remarks are applicable to general practice; but I specify them here as comprising the principles which may best regulate the use of depleting remedies in old age. Another important question regards the employment of Opiates at the same period of life: nor is this without its peculiar perplexities. Besides the modifications which are required from the different relation of years to bodily powers, and those further made necessary by the various idiosyncrasies as to opium, we have another specialty in this part of treatment from the changes which the functions of the brain, and often even its obvious texture, undergo in advanced age. Some of these changes are of nature to render the action of opiates ambiguous at least, if not in many cases distinctly hurtful. The vague dozing state, which steals gradually upon the aged, and, without any acute disease, seems in the end to turn life into a mere dream—this state expresses an altered condition of the brain and nervous system, in which narcotics probably quicken the change and render it more complete. Happily these are the cases in which

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they are generally least required. The state in question is a benign dispensation to the extreme term of life; expressing a gradual change without suffering, and rarely coming within the proper scope of medical treatment, unless there be some sudden passage into what is obviously a condition of coma. And even where other organs are more actively diseased, the sensibility in such cases is often so far lessened, as to supersede the remedies which are necessary at other periods of life to relieve irritation alone.

All questions of practice in these instances are manifestly those of degree; nor can any rule be given general enough to preclude attention to the individualities of each case. We have often, too, in old age other symptoms, which seem more directly to require the aid of opiates; such as habitual restlessness at night, under the influence of vague images and sounds, harassing the mind by a sense of reality beyond that of dreams. The effect of the remedy, however, even in these instances, is by no means an assured one; and benefit can be attested only upon trial. For progressive or permanent changes in the brain are generally concerned in such cases, and of a nature often to render injurious the stupor produced by narcotics, as well as to make uncertain their effect in procuring repose. There are doubtless cases where it is otherwise; but it is difficult to find other criterion than that of experience for determining these.

In the eneuresis of old age, especially when not depending on any disease of the prostate gland, I have often found more benefit from moderate opiates than from any other remedy. And in that harassing complaint, the prurigo senilis, which is ever so difficult of relief, this treatment is probably the best. Where the internal use of opium is objectionable on any account, the irritation may be greatly abated by lotions or ointments in which this is the main ingredient. Its combination with some mild mercurial, under the same form of use, frequently adds to the benefit obtained.

The principles as to diet in old age do not differ essentially from those applicable to every period of life. The questions are chiefly those of degree, and to be answered under the same modifications and exceptions as in other cases. Regularity in the times of food seems indeed, as in infancy, more essential to the well-being of the system than when the body is in the full

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vigor of life. It has now fewer resources, whether animal or vital, for righting itself: the functions are all less prompt to repair any partial injury or deficiency. Stimulants also, which either in kind or quantity might be injurious in middle age, are often fitting or needful at a more advanced period. The questions as to particular articles of diet may for the most part be settled on the same principles for both these periods; making due allowance for the difficulties of mastication in old age, and for the want of those active exercises of body which tend to obviate obstruction, and to assist all the powers, mechanical as well as vital, upon which digestion depends.

Here, however, as well as with regard to quantity of food, a qualification must be admitted for certain cases where the appetite and power of digestion are carried on unimpaired to extreme age; while other powers have decayed, or are utterly gone. In some of these cases it would seem as if a portion of the *vis vitæ*, lost or unused for other organs, had concentrated itself on those of digestion,—a view for which argument might be drawn from various analogies in the phenomena of life. It is difficult to explain in other way the disproportionate appetite for food which sometimes remains, when torpor has fallen upon every other bodily function.

All that relates to the preservation of warmth in old age, and, through this, of equable circulation, is too familiar to need being dwelt upon. The functions upon which animal heat depends have lost their vigor; and its production is in the same ratio diminished. We are bound with the greater care to save from loss by diffusion that which remains. This is important, not alone as sustaining the functions of the skin, the action of the limbs, and the general comfort, but also as lessening that tendency to congestion in internal organs which is one source of particular maladies, as well as a cause of general decay. We cannot restore the balance between absorption and deposition, in the loss of which some define old age to consist; but if these changes admit of retardation, as it is likely they do, this is probably best effected by whatever tends to maintain animal heat, and a free circulation throughout the vascular system.

Connected in some sort with this latter topic is the common question respecting old age (but one of deep interest, though common), to what extent the bodily powers may be preserved,

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by maintaining their assiduous exercise—whether, to take a single instance, the muscular organs may be longer kept in vigor by exertion to the full extent of ability at each successive period of life? or whether the powers, whatever they be, which minister to their actions, are longer and better maintained by comparative repose?

Though the inquiry may seem easily answered by common observation, yet is this not so. It involves some of the most subtle and obscure points in physiology; those, to wit, which regard the nature and origin of vitality, its mode of distribution and manner of decay—questions which, under different forms and various phraseology, have perplexed reasoning men of every age, and begotten theories and controversies without end. Taking life, however, not as a definite exhaustible quantity of an unknown influence, but as expressing a common result of the actions of many parts differently constructed and endowed, we are furnished with what comes nearest to a satisfactory reply. Whatever habits of living sustain the greatest number of organs or functions in healthy state (having regard also to the relative importance of these functions) may be considered as most conducive to length of life. The positive fatigue of any organ from its exercise must always be deemed an excess—of little import, it may be, in single instances; certainly injurious by frequent or habitual repetition. All exercise of a natural function of the body within this limit may be viewed, without material risk of error, as salutary in itself, and maintaining the integrity of the organ concerned longer than the opposite habit of inertness and disuse.[1]

This practical distinction is simple, and probably just. It is applicable, moreover, to every period of life, and very appropriately to old age in all its stages. The limit of action without exhaustion becomes more and more contracted, and in the same ratio must action be abated in degree. Exercise, short of this exhaustion, and with due allowance for casual fluctuations of power, may be admitted as maintaining the functions in their healthiest state, and thereby tending to lengthen the term of duration to each.

The same principle and rule may be applied without error to

[1] Cicero (*De Senectute*) has expressed this principle and rule in a few precise words: "Quod est, eo decet uti; et quicquid agas, agere pro viribus."

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the mental functions also. Here, equally, there is a power, naturally diminished in old age, susceptible of exhaustion from excess of use, and liable to be permanently impaired where this excess is frequent and severe in degree. On the other hand, disuse is not preservation; and we have every cause to believe that the integrity of these faculties is best and longest sustained by habitual exercise, within those bounds which are reached without fatigue at each successive period of life.

With all the desire that has existed to discover rules conducive to length of life and preservation of its powers, it is doubtful whether any can be found which are not directly subordinate to the principle just stated. We have no simple element, cognizable by our means of research, to deal with here. Organs and their functions are the sole interpreters of animal life to our present reason; and the maintenance of these in healthy action is all that can be done to solve the problem, and satisfy the desires which have been entertained by mankind of every age on this subject.

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CHAPTER XII.

ON THE EPIDEMIC INFLUENZAS OF LATE YEARS.

In the whole history of disease, there are few subjects of greater interest than those epidemics, which, under the familiar name of Influenza, have prevailed so frequently of late years;—with certain common characters not to be mistaken, and clearly identifying them with others which stand on earlier medical record. In a former chapter, I have mentioned the singular relation of these epidemics to other forms of disease; particularly to some of the exanthematous, intermittent, and continued fevers.[1] These connections, though in no case sufficient to establish identity, yet are intimate enough to suggest some community of cause, and a closer correspondence in every part than is expressed in our nosological tables. The pathologist and practical physician may both draw knowledge from this source. While to the more general observer, the epidemics in question are matter of deep interest, in their sudden appearance, in their wide but successive diffusion over vast tracts of the earth's surface, in their very general yet unequal influence, and in the amount of mortality they inflict.

Little can be added to the many exact descriptions we possess of the course and symptoms of this malady. What I have to say chiefly regards the question as to origin;—some particular points in the pathology of the disorder;—and a few others having reference to the principle and methods of treatment.

The first of these topics is one of exceeding obscurity, and upon which scarcely any certain knowledge has yet been ob-

[1] See Chapter "On the Connection of Certain Diseases." We have an expression of Sydenham regarding the whole class of epidemic diseases: "Ita ludit Natura in morborum epidemicorum generatione." It is the reasonable object of medicine, as a science, to annul this phrase; and for the "*lusus naturæ*," to substitute, in every possible case, the knowledge of a *natural law*.

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tained. The only opinion which can be stated with any assurance is, that there must be some peculiar virus of the disease, whatsoever its nature, which, like the virus of measles and scarlatina, is capable of diffusion and communication under certain laws; and is probably retained within the body a certain time, before evolving the more active symptoms of the malady. It seems hardly possible to avoid such conclusion, looking at the whole course and history of these epidemics. It is the only view under which we can explain their identity at different periods of time, and in distant places, during the course of the same epidemic. The influenza which occurred in 1675, and is recorded by Sydenham, entirely corresponds with our recent experience of the disorder. Huxham's reports of the epidemic influenzas of 1733 and 1743; Sir G. Baker's narrative of the influenza of 1762; the copious accounts we have from Fothergill, Baker, Heberden, Pringle, &c., of a similar epidemic which spread over England and Scotland in 1775; the remarkable report of the College of Physicians on the influenza of 1782, and the still more detailed reports we possess of that of 1803;—all these histories might be taken throughout as a correct description of the epidemics of our own time. Though the phenomena of small-pox and measles are more restricted, and of shorter duration, yet is their identity at different periods not more strongly marked than that of the particular malady of which we are now treating.

Again, the history of this disorder, as it appeared at Edinburgh, Copenhagen, or Berlin, will serve with little variation for its course and character at Lisbon, Madrid, and Malta; or even for its aspects in the southern hemisphere of the globe. This uniformity in localities so remote, and under conditions so different of climate and social life, can scarcely be explained but by identity of physical cause; and for such identity it is difficult to look to other source than some material virus;—recurring, we know not why, at different periods; and capable, through unknown laws, of being thus widely diffused, without change of character or loss of power.

It is true that some authors, and in concurrence with common opinion, have attributed these epidemics solely to atmospheric conditions, and the influence of extraordinary seasons upon the human body. And it may be admitted, on behalf of this opi-

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nion, that certain of the seasons during which they have prevailed, have been remarkable and anomalous; and further, that in common catarrh, arising from obvious causes of atmospheric change, many of the symptoms resemble the lighter and more transient forms of the disorder in question. This relation has, in truth, done much to embarrass the subject by attaching a common name to maladies which, if not really different in nature and origin, are so exceedingly different in degree and duration, as to make it impossible to submit them to a common description, or to similar treatment in practice.

To settle the question of dependence on mere atmospheric changes, look for a moment at the course and characters of the Epidemic Influenza, as we have seen the disorder under its most distinct forms in our own time. A disease which has appeared and spread at different seasons, in the middle of summer as well as in the depth of winter;—which has been found traversing whole continents, continuing this course through many successive months, and often assuming even a definite direction of progress;—which capriciously affects contiguous places in different degrees, and at different times;—which frequently continues in the same place for several weeks or months, under every appreciable variety of atmospheric state;—and which often affects, almost simultaneously, large masses of people living on the same spot, while others in closely adjoining localities are exempt;—such disease cannot be considered as due to any of the known qualities or variations of the atmosphere, to which the term weather is applied. If changes in the air are at all concerned, they can scarcely be conceived to act in other way than as engendering, or giving greater intensity, or readier diffusion, to some material virus, the real and active cause of the disorder. Even this is difficult to suppose under the diversity of conditions just noticed; while beyond this point we cannot go, without contradicting some of the most certain facts which enter into the history of the disease.

The argument and inference here may best be established by taking a simple epidemic; tracing, as closely as the report of facts will allow, the manner of its spread both as to time and localities; and removing all the conditions which we can thus prove not to be concerned. The influenza which spread over every part of Great Britain and Ireland during the spring

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of 1833 (after having previously appeared in Russia and the northern parts of Germany, inflicting great mortality in every part of its course), might well be selected for such illustration; were it not that the similar epidemic of 1837 was even yet more marked in its character and fatal effects, more widely diffused in its progress, and attested by more various record from the countries it traversed. I have given in the subjoined note certain well-ascertained facts regarding the epidemic of this year; chiefly illustrative of the points just mentioned, and in so far contributing towards the general history of the disease, and I would invite especial attention to them, for the reasons stated above.[1]

[1] The influenza of this year made itself distinctly known as such in London, the first week in January; the weather during the four months preceding having been singularly wet and stormy. On the 29th of November a hurricane occurred from the S. W., of almost unprecedented violence in this country; on Christmas day a storm of wind and snow, simultaneous over the whole of the west of Europe, snow falling even in the streets of Lisbon and Palermo; in such quantity in England as for some days to impede all intercourse throughout the country. What is more remarkable, from the remoteness of the locality, snow fell at Canton on the 8th of February, never before seen by the oldest inhabitant of that place. As another singular effect of this widely spread storm, the French army marching upon Constantine, in Africa, suffered severely under three days of incessant snow; while about the same time violent hurricanes prevailed over every part of the European and American seas. It might seem as if the earth, in its movement through space, had been exposed to other than the ordinary periodical causes acting on its atmosphere or surface. And it is yet for philosophy to say, whether certain of the more general conditions to which we are thus subject do not arise from material agents, wholly external to itself, with which our planet comes into contact or proximity, while sweeping along its orbit. Some sanction is given to this idea by the periodical meteoric appearances which have recently become the subject of observation; —by what is known of the varying extension and sweep of the zodiacal light;— and by other phenomena, the simultaneous apparition of which, on distant parts of the globe, would seem to preclude all local or limited cause of their production.

Succeeding to the Christmas snow in England was a severe frost for some days; then a thaw the first week in January, followed by very uncertain and fluctuating weather till the middle of February, this being the period during which the influenza chiefly prevailed; the winds variable and from every quarter; the barometer high and low; the thermometer ranging from 25° to 55°; fogs and clear atmosphere, though tending to the former; rains and dry weather, though the former much more frequent. It was a remarkable peculiarity in London, which I noticed also during the last weeks of 1836, that the heaviest and most continuous rains occurred with a high barometer; the longest courses of dry days with the barometer below 29°. Succeeding to the period of the influenza the cold was again very severe, with heavy falls of snow all over England, from the 20th of March to the middle of

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Some of these facts may on first view convey the impression of atmospheric causes being directly concerned in the phenomena.

April, and the thermometer as low as 10° or 15° below the freezing point during the latter days of March. Even in May the temperature was several times considerably below 32°.

The duration of the influenza in London might be stated broadly at six or seven weeks. I saw no new cases after the 16th of February, but the tendency to relapse continued long afterwards in one form or another. The greatest severity of the disorder was from the 20th to the 24th of January. Including the slighter cases, it would be no exaggeration to rate the number attacked at half the population of London (perhaps few escaped the influence altogether), while the ordinary ratio of mortality during this time was nearly doubled. The valuable report of Dr. Clendinning from the Marylebone Infirmary, and Dr. Heberden's Analysis of the Bills of Mortality during the epidemic, may be consulted on this part of the subject.

As respects the local progress of the disease, the following are a few of the facts I have noted, but which might be easily rendered much more numerous from authentic sources. The epidemic appeared in Russia, Sweden, and Denmark, during the month of December; at periods varying from one to three weeks before its occurrence in London. In Copenhagen, according to the memoir of Dr. Otto, at least 30,000 persons were under the disease at one time early in January. In Scotland, also, it was observed earlier than in England; and generally in the northern and eastern parts of England before the southern and western. It had been prevalent about a fortnight in London before it became so at Brighton and other parts of the southern coast. It appeared in Lancashire, Cheshire, Gloucestershire, and the southwestern counties, from a week to a fortnight later than in London. Though very general over the island, yet were there places seemingly unaffected, even in the contiguity of others where the disease greatly prevailed.

The epidemic showed itself in Paris about a month later than in London, having previously appeared at Calais and other intervening places. I saw patients who, on their journey from Paris to London, had come upon an infected town, and been suddenly seized with the malady. At Paris it was stated to have affected at least half the population, but seemingly with less mortality than in London. The epidemic spread gradually over other parts of France. About the end of February it affected the northern coast of Spain, the more conspicuous there from its influence on the events of the civil war, then raging in Biscay and Navarre. Almost at the same time it appeared at Lisbon—a new occurrence in that city—spreading *successively* to the several towns which lie upon the Tagus, even to the Spanish frontier. Dr. Leitao, who has narrated its progress in Portugal, seeks to show that it is contagious, and was brought directly from England to the British squadron in the Tagus, in the vessels of which it first appeared. The same idea prevailed in Biscay as to its manner of importation. The disease reached Madrid about the end of March, and prevailed there the whole of April.

In Germany the influenza appeared at Berlin in January, affected Dresden somewhat later, and Vienna and Munich a fortnight after Dresden. At Hamburgh, where it appeared in the first days of January, Dr. Rotherburg states that more than half of the population was attacked. I do not possess any information as to its progress in Italy. In Malta it first showed itself about the 1st of June.

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But when it is considered that these states of weather were similar and simultaneous over vast tracts of continent, while the disease made its progress by steps, having a certain course of general direction, but very irregular both as to space and time; —that it appeared at Madrid about three months later than in Scotland, and in Malta six weeks still later;—and that various intermediate towns and tracts of country seem to have been wholly exempted; we cannot reasonably look to weather as a direct cause, whatever its influence in giving action or diffusion to other materials of disease.

Amongst other particular proofs to the same effect, the following may be cited as especially remarkable. The influenza of 1831 began in London about the middle of May, and continued during June and a part of July, under very hot though damp weather. The epidemic of 1837, still more violent in degree, appeared during the first days of January at the close of a severe frost, and prevailed till towards the end of February; followed again by weather of singular severity. Again, with respect to this latter epidemic, we have the fact of its raging in England and the northern parts of Europe during the depth of winter; while its prevalence in some southern countries of Europe was extended to the latter end of May, or the beginning of June.

I may mention as another evidence of the same kind, and interesting from the remote part of the globe which furnishes it, that a severe influenza, having all the characters of the European epidemic, prevailed in Van Diemen's Land in December, 1838; the season, therefore, corresponding with the month of June in our hemisphere.

Recurring to former periods, the influenza of 1762 appeared

A remarkable fact is, that an epidemic, having all the characters of the influenza of the northern hemisphere, prevailed at Sydney and the Cape of Good Hope in the latter part of 1836; the time thus corresponding with its earliest appearance in the north of Europe, though under a date of *season* wholly different. Sir John Herschel informs me that the weather was warm and apparently genial, at the time when almost every individual in the Cape District was suffering under the epidemic. The malady spread up the country as far as Gnadenthal, producing there considerable mortality in the Hottentot population.

Equally remarkable is the fact recorded by Governor Simpson, that the influenza prevailed largely and fatally in the Hudson Bay territory, among the Chippeway Indians, during the autumn of the same year; continuing even until late in the ensuing winter among the natives on the Bear Lake, under the Polar Circle, and in the coldest point of the American Continent.

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in Poland, the north of Germany, and England, in February and March, and raged on the shores of the Mediterranean during the heats of summer. The epidemic which spread over England in 1782 during April and May, had been noted in the East Indies during the latter months of 1781; had prevailed in Russia from December to February; and did not reach Italy and Spain till the autumn of 1782, three months after its prevalence in England. On the other hand, the influenza of 1803 took, as regards Great Britain, a general course from south to north; raging in Paris some time before it appeared in England, and occupying three months in its gradual but capricious advance to the northern parts of the island.

It is impossible to look fairly at these circumstances, and not to see that the known conditions of the atmosphere, as we estimate them by our instruments, are wholly inadequate in explanation. If any fact were favorable to the hypothesis in question, it would be the more than common prevalence of easterly winds, with a hazy atmosphere and fogs, during the season of these epidemics; the latter condition especially having been noted at many different periods of their occurrence. Though correct observations are much wanting on the subject, something also may perhaps be assigned to the electrical state of the air, manifestly disturbed during some of these periods, and possibly becoming in this disturbance the cause of the phenomena just noted. Still these circumstances are too partial, and too often occurring without like concomitant effects, to justify the belief that they act as direct causes of the disease. The most summary statement then of the argument is this;—that all obvious conditions of weather being the same in a certain number of places, the disease appears in them at very different times, or in some not at all;—and secondly, that it occurs in various places, or in different years at the same place, under states of season and weather wholly opposed to each other.[1] The conclusion derived

[1] Sir G. Baker, though relating certain singularities in the winter and spring of 1762, yet is wholly opposed to the view of these being directly concerned as the cause of the epidemic. Amongst other objections to this, he states the fact that places within two miles of London were attacked much later than London itself. He remarks further, what well merits notice, that the disorder appeared in the metropolis before it was known in any other part of Britain. It began here the first week in April, in Edinburgh a month later, in some parts of the north of England not before the end of June.

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from these facts is only of negative kind, but nevertheless important as putting aside the error of a popular opinion, and clearing the ground for other and more positive results.

Arguments of the same kind, somewhat modified, apply equally to the notion of what has been vaguely termed terrestrial influence—an undefined agency proceeding from unknown source within the earth itself. In one instance, the simultaneous appearance of a similar epidemic in the northern and southern hemispheres might seem to give sanction to such hypothesis; but this instance, as far as we know, is a single one, and cannot be placed in opposition to the many facts attesting the slow and irregular progress of the disease even through adjacent localities, under conditions apparently the most inconsistent with this idea of its origin.

I have dwelt so far in detail upon these questions, not merely from their intrinsic interest, but from the connection with many other important points in the history of disease. Whatever the causes of influenza, it is clear that they are closely associated with conditions which operate in producing or giving greater frequency to other disorders also. It is most important to estimate the nature and extent of these connections, and the illustrations which they mutually afford. And no disorder furnishes such information more remarkably than that of which we are now treating.

It is impossible that the relation of these questions to those which bear upon the Asiatic Cholera should escape notice here. Independently of some singular concurrences as to time, the question regarding dependence on atmospheric causes is so much alike for the two epidemics, that any argument admits of being directly translated from one case to the other. The symptoms indeed are far less analogous than the manner and extent of diffusion of these erratic disorders: but even here there are certain resemblances in the suddenness of invasion, and the rapid or sometimes instant prostration of power which ensues. All the

Before quitting this subject, I must refer to a very valuable memoir by Dr. Black, of Bolton (see *Seventh Report of the British Association*), on the epidemic of 1837, as it appeared in that town. This paper, besides other important statistical details, contains a meteorological register for the first three months of the year; from which, however, no especial inference can be drawn in relation to the periods or peculiarities of the disease.

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proofs we possess of a virus peculiar to the cholera equally apply to the epidemic influenza; and the need of such specific agent to explain the facts is scarcely less than in the case of scarlet fever or measles, though there is no evidence of contagion here as complete as in the latter diseases.

I have already noticed the argument to this effect from the identity of the disorder at different periods of its occurrence, and in distant countries; and the force of this becomes more apparent on consideration of the various details of time and locality given above. On careful comparison of my notes during the several influenzas which have occurred since 1831, and extending the comparison to those best described of former periods, I cannot doubt that every essential character of the disorder is the same; and that there must be identity of material cause to explain this fact. It seems impossible to suppose any series of circumstances, really different, yet thus capable of producing and disseminating the same exact character of disease; and under conditions which needfully imply a continued and fresh production of the morbid cause. The near equality as to periods and duration of the disorder in each given place is a strong collateral argument; a correspondence extending even to the period which it has generally occupied in passing over the different countries of Europe. Were other argument wanting to establish identity of cause, it might be found in the remarkable sequelæ of the disease, to which I shall afterwards advert.

While admitting, however, as almost necessary, a specific virus or matter producing the disorder, we are scantily provided with facts to indicate its manner of generation and spread—why it should be produced at periods so irregular as to interval—and what is the precise nature of its action on the human body. Still these difficulties are not greater than occur in the case of the contagious exanthemata; where but for the familiar view of infection (an explanation itself of recent date in some of these disorders, and which gives the aspect of more knowledge than it really conveys) we are equally ignorant of the nature of the animal poisons concerned, or of their manner of action. That the virus, if not generated by certain conditions of atmosphere, derives activity or power of diffusion from them must be admitted as probable; and it is under this view that states of weather may be considered to have influence, and not from

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changes in the obvious qualities of the air operating directly on the body. The distinction here is obvious, and best accords with the facts already noted in the history of these epidemics. It is, moreover, more satisfactory to reason than the vague phrase of an epidemic constitution of atmosphere, which can be understood only by referring to the points on which such distinction is founded.

In another chapter I have stated the arguments which may be alleged for the hypothesis of organic or animalcule life as the source of the Asiatic Cholera. It is obvious that many of these arguments, if valid, must apply equally to the epidemic influenza now under our notice. The diseases, indeed, being specifically different in symptoms, must have different causes and origin. But the manner in which the cause operates in each case, in the progress and diffusion of the malady, is so singularly alike, that it is impossible not to suppose some analogy or relation; and such may best, perhaps, be found in the hypothesis just named. I will not, however, press the argument here, it being enough to state in one place what is still speculation only, and the Cholera furnishing a better instance than any other on behalf of the opinion in question.[1]

Without reasoning further, then, on the nature of a material agent, which, if existing, is yet removed from all direct observation, the question may fairly be entertained, whether this disease of epidemic influenza is contagious in the ordinary sense? And, further, whether the virus, when received, has a period of incubation, like the infectious matter of some other disease?

On the first of these questions, the opinions of medical men have been for the most part negative, though without much inquiry directed expressly to the subject—a singular fact, seeing the many curious analogies between the manner of diffusion of this disorder and of Cholera; and the active controversy that has existed as to the contagiousness of the latter. Any proof respecting the influenza is indeed made difficult by the greater and more rapid diffusion of the malady—by the resemblance of

[1] There seems sufficient evidence that, among other animals, horses and dogs at least are frequently subject to a disorder resembling influenza, during the period of its prevalence in any given locality. The influenza of 1840, though comparatively mild in its character, was attended by a general and somewhat severe epidemic among cows and sheep.

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its lighter forms to catarrh, and the connection of those of graver kind with other diseases—and by the greater effect of idiosyncrasy and collateral causes in modifying the whole course and aspect of the disorder. Little more can be affirmed on the subject than that we have no distinct proof of infection from one person to another; but at the same time no evidence directly against the opinion, and some presumptions for it.[1]

To the world at large it may seem strange that this and similar questions should still remain in state thus equivocal; and that so much controversy should still exist as to all that concerns infection in disease. Those accustomed to deal with medical evidence in practice can alone understand the difficulty of attaining results on such subjects free from ambiguity. If the point be better determined hereafter, as regards influenza, it will probably be found that the atmosphere is the medium in spreading the virus; and that modifications are derived, as well from changes in this medium, as from concentration of the virus in particular situations. The latter condition brings us nearest to the view of personal infection; and the kind and amount of evidence are much the same as in the case of certain fevers of typhoid type, to which these influenzas are in so many ways closely related.

It is, from similar causes, no less difficult to ascertain unequivocally whether the matter of the disease is dormant for a time after received, like that of certain other infections. The impression I derive from a comparison of many instances is in the affirmative. It must be owned that there are various cases from which an opposite inference might be drawn. Such, in particular, are those where the disease seems suddenly to attack persons arriving at a place in which it is already prevailing;—those also, well attested, where in a prison, barrack, or hospital, a large proportion of the inmates sicken almost at the same time;[2]—

[1] The scantier and more tardy intercourse between distant places in 1762 made the evidence at that time more explicit than at present. Sir G. Baker remarks that at several towns, Norwich, Lincoln, Leicester, and Exeter, the first cases occurring of the disease were persons recently arrived from London.

[2] In one of the London prisons fifty or sixty persons fell ill on one day. The same thing happened with respect to regiments both in London and elsewhere; and this is especially remarked as having occurred in several of the epidemic influenzas of the last century. I have often, in private practice, known six or eight persons seized in a house within twenty-four hours. Still all these instances may be reconciled to the supposition that a certain time is required for the disease, actually received, to evolve itself in an active shape.

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and, further, the remarkable examples, frequent during the more severe of these epidemics, where the patient, seemingly well at one moment, is at the next laid prostrate under the disorder; a seizure scarcely less rapid than that of cholera, though less dangerous in kind. The former cases, however, are somewhat ambiguous in kind; and, for the latter, analogy may be found in many conditions of fever and exanthematous disease, where the onset of the symptoms is equally sudden, though an interval of many days has succeeded to the original action of the exciting cause.

We have further argument for this view in the length of time during which liability to recurrence of the disorder exists in the constitution; either with the same symptoms as in the first access of the malady; or more frequently under some of the modifications already adverted to, and particularly that of intermittent fever, with various local affections of anomalous kind. The intervals between such recurrences, though rarely perfect, yet are well enough marked to denote explicitly the changes taking place. A peculiar form of angina, a sort of dusky red band stretching across the tonsils, and giving a sense of constriction and difficult deglutition rather than pain, is among the symptoms most prone to return; and this disposition may often be taken as an index of the time during which the patient is under the epidemic influence, whatever this may be. At the periods when it tends especially to an intermittent form, I have known the angina to come on at stated times every day, for many days together, with intervals of almost entire disappearance between.

It is a question full of curious import in what this disposition to recurrence consists. If analogy could solve it, we might find this, very closely in every sense, in the phenomena of common ague; equally recurring at repeated and often distant periods from the time of the original attack. But, intimate though this relation be, it translates the difficulty rather than solves it. We are totally unprovided with facts in either case to show the nature of this singular liability of the body, maintained even where no obvious symptoms are present. Looking to the great and sudden prostration of power, which is so frequent a symptom of influenza even in its earlier stage, it might be conjectured that there is a state of the nervous system, giving proneness to

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return of the disorder. Or it may be that the relapses, whatever form they take, are due to the virus of the disease still present or recurring; and acting on the body under modifications, either in its own nature, or derived from changes in the media of communication. Still these are terms too vague to advance us much in real knowledge. We must regard the question as one among the many to be committed to future research; aided perchance by the discovery of new relations between external agents and the human body, which may illustrate equally the phenomena of health and of disease.

Their long-persisting influence upon the constitution is certainly a remarkable point in the history of these influenzas. When the disorder has been severe, this state often distinctly continuous for several months. But even where so slight as scarcely to be defined at any one time, there yet is frequently observable a condition of body, prone to various ailments and disturbances, all having relation more or less to the same general cause.[1] These slighter cases, testified chiefly by such liability, and lasting sometimes for a considerable period, are instructive in many ways. It is the same virus of disease; enfeebled in its action, either by lesser quantity or intensity, or by the difference of bodily constitution to which it is applied. Each of these modifying causes must in fact exist; and both are doubtless concerned in some of the singular varieties which the disorder assumes; connecting it with other maladies by relations which deserve the most sedulous observation. Several of these relations I have pointed out in the chapter on the Connection of certain Diseases, to which reference has already been made.

The amount of virus (expressing in this term what may be due to concentration as well as quantity) is obviously a very important circumstance, and not enough regarded in common

[1] Among other sequelæ of Influenza, I find in my note-book the record, in the summer of 1841, of a singular frequency of sudden attacks of angina, following immediately upon one of these epidemics;—swelled glands about the throat, rapidly receding and others appearing—febrile symptoms, with intermittent tendency—vertigo—general prostration of power;—these symptoms so uniform in character, though various in degree, as to prove the certainty of a common cause.

The advocates for atmospheric or telluric causes of disease might find some argument in the incidents of this year, to the extraordinary character of which, as marked by hurricanes, earthquakes, and rapid changes of heat and cold, I shall have occasion to refer to in another Chapter.

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reasoning on the subject. We may argue here almost as securely as if we knew the actual nature of the material cause. Whatever it be, quantity and intensity must, according to every analogy, have their appropriate effect; and the modifications so produced, though they cannot be specified in our present knowledge, yet are certain in their existence, and form the proper objects of future research.

There is difficulty in discriminating as to the relative influence of the two conditions mentioned above; viz., the individual susceptibility of the patient, and the amount of virus present and active at the time. One evidence, but this only of general kind, may be found in the proportion of severe cases of the same character, existing at the same period. When this is large, the inference may be drawn that the material cause of disease is abundant and active on the spot. Where but few urgent cases occur in relation to the extent of the disorder, there is fair reason (though obviously not conclusive) to attribute these to the particular state of the individuals so affected. The same general rule may be applied in all diseases where a specific virus is presumed to exist; qualifying it of course by a regard to those local and partial concentrations of this virus, which doubtless occur wherever it is present as the cause of disease.

Without going into any needless history of symptoms now so well known, I may notice two or three points especially characteristic of these influenzas, and at the same time indicating their relation to other morbid states of the body.

Perhaps the most remarkable of these is the sedative influence of the disorder; exceeding in degree what belongs to common fevers;—testified in the graver cases by a sudden and often extreme depression of the vital powers, even as the first obvious symptom of the malady; with rigors and congestion in the internal organs, and feeble reaction succeeding to this state;—in other cases, less severe, by a speedy passage of the early and more active febrile symptoms into those of low fever, with something of a typhoid type;—in cases slighter still, by a prostration of strength out of proportion to the other symptoms present, and continuing after they are removed. This depression in different degrees extends in remarkable manner to the whole nervous system; and the mental functions and feelings are im-

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paired almost in the same ratio with those of the body.[1] The latter effects seem especially to occur where the attack of the disease is most sudden. I have seen cases where there was rambling and indistinct utterance at the very outset;—others in which the state was one of vague stupefaction;—some attended with a sense of urgent danger at the moment of seizure. Not unfrequently these symptoms came on suddenly in the middle of the night. A very common occurrence in the disorder is the failure and confusion of memory; obvious to patients themselves, and often continuing, in one degree or other, through its whole course.

The tendency to the congestive or typhoid form has shown itself particularly, as I think, at the time when the epidemic was at its greatest height in the number and severity of the cases; and when therefore the virus might be presumed most active in its influence. This warrants the conclusion that its direct effect is sedative upon the body; and such inference is confirmed more or less in all the forms the malady assumes, and best explains its various anomalies, as well as the relations it exhibits to common fevers of the typhoid kind. Though identity of nature or origin can in no wise be proved from these relations, and though the material cause must be presumed really distinct, yet the analogies become more remarkable in proportion as they are closely pursued. This, moreover, is one of the cases where knowledge may equally be drawn from studying the differences and the resemblances of disease. The exact correspondence of some symptoms,—the partial or entire deviation of others,—afford a sort of practical analysis, capable probably of being reduced to a more definite form than has yet been given to it; and of great value in a science, where strict evidence is for the most part of such difficult attainment.

The reaction which in the influenza succeeds to this first prostration, though manifest in most cases, yet becomes so under the same general aspect of depressed powers. There is fever, often of some days' duration, and attended with great restlessness, sometimes with low delirium, but at the same time

[1] "In omnibus animi demissio, virium que defectus, major longè quam pro morbi ratione."—*Georg. Baker Opusc. Med. De Catarrho Epidemico* anni 1762. The same remarkable fact is noticed in almost every one of the histories of the Epidemic Influenza, to which reference has been made.

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little vigor of circulation, and continued perspiration, with no obvious relief to the symptoms. The pulse speedily loses any quickness it has had in the outset, and may often be felt beating sixty or seventy in the minute in an adult, who is suffering at the moment under the violent paroxysms of coughing which attend the disease, with pain, constriction, laborious breathing, and every other symptom ordinarily marking inflammation of the chest.

This latter condition is another token of the adynamic state of the system—one scarcely capable of maintaining true inflammation of parts, yet simulating the character of it, if such phrase may be used, under ignorance of the true nature or proximate cause of the affection. The more common catarrhal symptoms of the disorder, the peculiar irritation of the fauces, windpipe, and bronchial tubes, the corresponding irritation of various parts of the alimentary canal, from the œsophagus downwards, all show the same generic character and type. The mucous membrane lining these passages seems to be the texture primarily, perhaps chiefly, affected, including, of course, the glands so numerously spread over it, the secretions from which are altered both in quality and amount. If a name were required to mark the nature of the inflammation, it might best perhaps be termed erythematous or erysipelatous in kind, having manifest relation to some of the various forms of these disorders. But it is safer in our present knowledge to treat of it as specific, seeing the singular nature of the disease, the manner and extent of its diffusion, and the strong proofs of a specific miasma as the source of the whole.

Few things about the influenza are more striking than the contrast just noticed between the seeming severity of local inflammation and the actual adynamic state of the body; and none is more perplexing to the practitioner new to the disease, or inattentive to its peculiarities. The violent and painful cough attending it is the most striking example of this. And here we may best recur to the analogy of hooping-cough, there being, together with some essential differences, certain points in which the disorders show curious relation to each other.[1] These are

[1] In the chapter "On the Connection of certain Diseases," I have noticed the frequent prevalence of hooping-cough concurrently with the epidemic influenza, and the difficulty in many cases of distinguishing the two disorders.

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the cases where bleeding is either useless or deeply injurious, and where cordials and bark are sometimes an effectual remedy for symptoms apparently the most opposed to their use. The pulse, as I have already noticed, is the fairest interpreter here, and no material error can arise in practice, if it be adequately consulted. Should we admit the term pneumonia, to describe some of the cases where the lungs are chiefly affected (and it is scarcely possible to avoid altogether the name), it must be taken with qualification annexed. The condition is probably nearest akin to that form of pneumonia called typhoid, which is generally concurrent with low fever, and, in fact, has been singularly frequent in the fevers of this type following the spread of the epidemic influenza.

Another point (already in part noticed) of great interest in the history of the disease, is the variation of the parts affected in different individuals, or at different periods in its progress. I have elsewhere adverted to the importance of this consideration in all our reasonings on the communication and connection of diseases. It is certain and obvious (though as a principle by no means sufficiently recognized) that the same virus acting upon different parts of textures, from variations in its own intensity or manner of application, or from idiosyncrasies in those receiving it, may produce an aspect of symptoms very different in kind as well as degree. In the earlier stage and slighter degrees of the influenza, the action of the specific cause seems to be chiefly on the membranes which are the seat of common catarrh, producing symptoms which in various ways resemble this complaint. There is every reason however to presume that the relation here is not closer than that of the same surfaces undergoing irritation from causes specifically different. The connection with that singular form of catarrh (if so it may be termed) to which the name of Hay Fever has familiarly been given, is manifestly more intimate; and especially in what regards the tendency of the latter to intermittent character. But in truth the whole subject of Catarrh is perplexed by nomenclature, and by a confusion of causes and symptoms, justifying, even at this day, the phrase of *Catarrhi Deliramenta*, which Van Helmont formerly applied to it.

In a more advanced stage, or under exposure to a more active virus, or from difference of bodily temperament (for it is not

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easy to fix the relative influence of these conditions), the disorder extends over the membrane lining the air-passages of the lungs; and down the œsophagus, more or less extensively, towards the stomach and alimentary canal; producing large mucous secretions, cough, and laborious respiration; and often that peculiar distress about the præcordia which we find in some eruptive diseases, before translation has taken place to the skin. As these symptoms occur sooner in proportion to the severity of the illness, there is reason to attribute them to greater activity or a more diffused influence of the virus, rather than to mere spreading of the inflammation first induced. And the same reasoning will apply also to those singular cases where the constitutional effects of the virus are more sudden and complete; bringing on, almost without intervention of other symptoms, a typhoid state of the whole body. I have already alluded to the greater frequency of such cases at the period when the epidemic is at its height; and when therefore the material cause may be presumed in greatest activity. I have been led by observation to believe that in these instances the more direct and extensive effect is on the membrane of the alimentary canal; an inference according well with the many cases in which affections of this membrane bring on, or are blended with, fever of a typhoid character.

The form of the malady, last mentioned, is by no means limited to the aged, or those of feeble temperament. I have seen it quite as often, and equally severe, in persons of strong constitution and middle age; and hence may be found further reason for concluding that the variation is in the intensity of the cause applied. The sudden stupefaction or low delirium of these extreme cases is represented in those of slighter kind by the weakness, nervous depression, and great restlessness at night; all characteristic of the remarkable effect of the disease upon the nervous system.

The state of the tongue and fauces; the frequent occurrence of angina, with a peculiar kind of dusky efflorescence rather than any considerable swelling; the sense of heat or excoriation passing down the œsophagus into the stomach; are other symptoms indicating the direct effect on these membranes, even more certainly than on those of the respiratory passages.[1] And

[1] Both Huxham and Baker describe the white creamy mucus with which the tongue is covered in this malady, and the observation agrees with what we see in

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a like inference may be drawn from the eruption or efflorescence on the skin frequently attending the complaint; such symptoms, as well as the herpes labialis, being a very common index of the transference of irritation from the internal membranes to the surface.

I may note here a fact, which I do not think to have been sufficiently adverted to. I mean, the aggravation of distress to the patient, when the disorder, as so often happens, passes suddenly from the condition of catarrh, to that of an affection of the intestinal membranes. Though apparently a simple translation of place, without alteration of the cause of disorder, yet the symptoms assume a new character, both to the patient and others;—in this furnishing a proof of what I have elsewhere stated, that the same morbid cause acting upon different tissues, or even on different portions of the same tissue, may produce such diversity of effect, as to give the aspect of other and often very distinct forms of disease. The proof in this instance is made more complete by the disorder frequently reverting again to the catarrhal form; with the cessation of the symptoms which had marked its translation to the membranes of the alimentary canal.

Of the symptoms directly consequent upon influenza, if not indeed to be regarded as an integral part of the malady, one of the more frequent and remarkable is the tendency to neuralgic intermittent pains or morbid actions in different parts of the body; with periods often as regular as those of common ague, and sometimes taking the tertian as well as the quotidian type. I have noticed certain of these curious facts in another chapter, when treating of intermittent actions of morbid kind; and I need not further particularize them here, though they well deserve to be studied in the relations they express between different forms of disease. Beyond these general relations we have no means of proceeding. Little connection, save that of priority and sequence, can be traced between the early and more familiar symptoms of influenza, and such partial intermittent affections. Whether they depend on idiosyncrasy of the patient, or on some peculiar application of the virus, or simply on liability

the more recent influenzas. But I do not find noticed by them, or others, the peculiar and vivid redness of the point of the tongue, even when thus coated elsewhere; a mark so frequent as almost to be discriminative of the disorder.

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given to be affected in this way by other causes, are points equally obscure. They seem to be more frequent when the epidemic is declining in any given locality; as the typhoid character of the disorder is more common and distinct when the influence of the morbid cause is apparently at its height. But, however these questions be decided, a reference to them must enter into all our reasonings on this part of pathology.

I have also noticed in another chapter the frequent occurrence of dysenteric symptoms (often a well-marked form of this disease) during the progress of the influenza; or, it may perhaps more justly be said, in sequel to the ordinary course of the epidemic. The description by Sir G. Baker of the very severe dysentery which prevailed in London, immediately after the influenza of 1762, is well known.[1] The facts here are so well established by their repetition, that it is difficult not to suppose a direct connection, either in the nature of the cause, or in a liability created by the prior disease to be affected by the ordinary causes of the latter. I know no decisive evidence on which to determine this point. The latter, perhaps, may be conceived the more probable, seeing the apparently increased liability to other disorders, also epidemic or contagious in kind, which exists either during the prevalence of the influenza, or as an effect of the state of constitution it leaves behind. The tendency to gastric fevers, and other disturbances of these viscera, is, however, that especially marked; and we have probably no record of the epidemic influenza largely prevailing, without its being followed speedily by some other epidemic of this character. I have already sufficiently dwelt upon the importance of noting all such relations in practice.

Any remarks on the medical treatment of influenza must be prefaced by the consideration, that we have here a disease depending on a specific cause, to meet which we possess no specific remedy. We are notoriously unprovided with any means to resist the access of this cause; or to remove it from the body, when received and in action there. Nor is there any explicit proof that we have the power of shortening the period of its action, or altering its course and direction as

[1] I am led to infer from Sir G. Baker's narrative of these two epidemic diseases (if they may be spoken of thus separately) that the influenza was less virulent than those of 1833 and 1837, but the dysentery much more general and fatal than any of recent occurrence.

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regards particular parts. What alone can be effected in our present knowledge is to watch over the symptoms severally; to mitigate their excess; to promote a healthy and sufficient state of the natural excretions; to obviate injury to any particular organ or function; and to restore the strength by such remedies as are permissible for this purpose.

These circumstances need to be kept before the mind of the practitioner. It is very important for the right conduct of practice, to know as far as possible in each case the exact reach of our power. Much fruitless or injurious interference is thereby prevented; and a better chance afforded of attaining eventually the objects which are thus clearly defined as wanting to us. While extending more or less to the treatment of every disease, the precept especially applies to those where there is a specific virus or definite cause of morbid actions, for which we yet know neither preventive nor specific antidote. Here the officiousness of active practice is often a source of much mischief; and the prudent physician will reserve it chiefly for the conditions just stated, where alone it is justified by reason and experience.

A question, however, may be raised regarding the influenza, whether the disorder does not admit of being arrested in the outset: and common opinion, founded on cases where the symptoms speedily subside after their first appearance, leans towards this belief. The proof, however, is ambiguous, seeing the variations which may arise from different states of the virus, or the temperament of the patient; and the frequency of such sudden subsidence occurring under different remedies, or where none whatsoever have been employed. If I were from my own observation to name any remedy, having some pretensions to the effect, it would be an active Emetic in the earliest stage of the symptoms; but for the reasons just given I am far from thinking this an assured fact.

Whether so or not, vomiting is, I believe, the best means we can employ at the commencement of the disease, as well as under different conditions of its progress. That it should have been so insufficiently used, in the treatment of the influenza in this country, must be attributed to the same causes which have made the employment of Emetics comparatively so infrequent in modern practice. In clearing the mucous linings, both of

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the air-passages and stomach, from the secretions which load them, no remedy is of equal avail; and none safer in its application, or more immediately attesting the good that has been obtained.

The employment of purgatives in influenza is probably best regulated by the principles which direct their use in the exanthematous fevers. Frequent and violent action of this kind I believe to be useless, or positively injurious, in the disorder; having no specific influence as a remedy, and tending to increase irritation and depress the general powers. This is probably equally true when the pneumonic symptoms are most severe, and where the virus affects chiefly the membranes of the alimentary canal. The cough which is so peculiarly characteristic of the disorder, whether proceeding from either or both of these sources, is often manifestly increased by the irritation thus given; while the bowels, under excess of this practice, readily become tympanitic, and more fretful and disordered in their actions.

These comments of course do not apply to a better regulated use of the remedy. The proper use of purgatives in influenza seems to be to clear the bowels thoroughly in the outset; and subsequently, by such as are simple and least irritating, to prevent accumulation or disordered secretions. Nor does this view exclude the employment of mercurials to correct and maintain these secretions in healthy state. They are, in truth, among the most efficient aids we possess in the treatment of the disorder. However it may happen at the beginning, the liver usually gets gorged, and its functions disturbed, in the progress of the malady; and this is true even in many of those slighter cases, where the patient, though for weeks together under the influence of a morbid cause, is never actually withdrawn from his ordinary occupations. To relieve this state, mercurials are the speediest remedy; and often more effectual, generally less irritating, when uncombined with other cathartics. So employed, with the intervention of stronger doses occasionally, calomel and blue pill act most beneficially; not solely in relieving present symptoms, but in preventing many of the most distressing sequelæ of the disease. And this is an effect which, in my experience, is by no means equally produced by mere purgatives, frequently or harshly employed.

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I have never known any certain benefit from the treatment of influenza by sudorific medicines. Ambiguous in use in other cases, they are still less called for in a disorder of which continued and copious perspiration is one of the most frequent symptoms. If antimonials are of any avail here, it is not, I believe, under this view, or even as a common febrifuge, but solely as one means among others of exciting vomiting, where this is desirable.

The same opinion may be fairly given as to the whole class of expectorants. I never saw any good obtained from them in the influenza, unless when unintentionally they have acted more or less as emetics. The act of vomiting is in truth the most effectual expectorant in most cases where this relief is urgently required.

It seems necessary to add opium to the list of medicines from which we can obtain little certain benefit in this disorder. The restlessness at night, so common as one of the symptoms, would seem to require its use; but from some cause or other it is rarely effectual in giving much relief. Although the cough may for awhile be mitigated or suppressed by its action, it is doubtful whether the good so obtained is an equivalent to the disadvantages in other ways incurred.

The most important question of treatment in influenza doubt less regards the extent to which antiphlogistic means may be carried, or the fitness of employing them at all. And the point as to *bleeding* is that which stands foremost here, and has chiefly embarrassed all practitioners. The most general precept on this subject is liable to exceptions; but, collecting what on the whole is safest and most expedient, it must be one which distinctly forbids bleeding as an ordinary practice in the disorder. The adynamic type throughout in the greater number of cases;—the singular disproportion in all between the seeming severity of the inflammatory symptoms and their real slightness or nullity;—the actual failure of bleeding in mitigating the violent and painful cough which seems most expressly to require it;—and the frequent success of remedies precisely the reverse of this;—all show a specialty in the disease, to which we must refer, more or less directly, in every question of practice. Whatever the cause or precise seat of irritation, it is certain that it has rarely the characters of true membranous inflammation. In truth, the

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same reasons which prevent or limit bleeding in hooping-cough apply no less to the peculiar cough and irritation of the influenza. We have rarely any authority for it in the state of the pulse; which neither in strength nor frequency bears relation to these seemingly inflammatory symptoms. While the difficult or painful respiration, which often suggests the remedy, furnishes evidence against its fitness, by becoming frequently more laborious than before;—the effect of larger accumulation in the bronchial cells, and of diminished power.

These are all points of great interest in practice. It is less needful indeed to enforce them now, as the repeated experience in several epidemics, speedily following each other, has given general knowledge and warning on the subject. It is certain, however, that much injury resulted from the error, before this experience was obtained; and especially in the treatment of those of infirm habit, or who were advanced in years.

Exceptions there undoubtedly are to the principle just laid down. In some habits, prone to inflammation, the first reaction of the disorder may pass into what is really this state, and require bloodletting for relief. There are other instances where the congestion on the lungs is such in degree as to demand the same remedy. And I believe on my own experience that these latter instances are more frequent than the former, and carry with them more distinct justification of its use. It is hardly necessary to add that, when blood is taken away, it should be as early in the disorder as possible; with a cautious regard to age, and with close observation of the effects on the pulse, cough, and respiration. Should there be any doubt on the subject, blisters may expediently be tried first; and especially where the cough is the symptom most urgent. The epigastrium will be found generally the best place for their application.

The conditions which thus render bleeding for the most part improper, go further, in sanctioning to a certain extent the treatment which has for its express purpose to excite or sustain the vital powers. And accordingly cases are not infrequent where stimulants give instant relief to symptoms which, on first aspect, and interpreting them by other diseases, would seem to require very different treatment. Here happily, as I have before observed, the pulse is generally a more faithful guide; and there are few cases where, if fairly consulted, and in immediate sequel

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to the means employed, it may not be relied upon for direction. If wine or other cordials give it force and steadiness, without adding to its frequency, and without exciting fresh cough, the inference may safely be drawn in favor of this treatment during the further progress of the disorder.

But watchfulness as to these tests is always needful, to guard against exceptions to the general rule, and excess in the methods so suggested. Stimulants may be used earlier, or to greater extent, than it is fit they should be; and morbid actions brought on which would not otherwise have existed. It is well indeed to repeat here regarding the treatment of influenza, that the majority of cases require rather the forbearance of the physician than any strenuous use of active remedies, upon whatever principles adopted. Still it is important that these principles should be fixed as far as possible; and made capable of ready application to the more urgent, though rarer, cases which come before us.

The only other remark I have to offer regards the use of bark, or the sulphate of quina, in the influenza. Its value here is unquestionable, derived not simply from its quality as a tonic, but further, and more especially, from its specific power in various intermittent affections. Any inference that might be drawn as to its use, from the tendency in the disorder to these intermittent actions, is fully justified by the actual effects. It relieves them, when fully established, almost as speedily and certainly as the attacks of a common ague, and this whatever the part of the body so affected. This remarkable power over one of the conditions of the disease gives so far a specific character to the remedy, that it may rightly be adopted in prevention of a state which it is capable of curing. It is not easy, however, to define the exact time at which its use shall be begun. This must vary in different temperaments, and in different degrees of the disorder. Occasionally the irritable state of the membranes of the alimentary canal creates a hindrance requiring either precaution or delay. But it is an inference from the reasons already stated, that bark may be given safely and beneficially in many cases, where there is still hard cough, with pain, oppression, and general distress—and experience confirms this conclusion. A soft feeble pulse and moist skin often concur with these symptoms, and furnish additional authority for the practice. If the cough itself, as frequently happens, tends to intermittent character, the

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security of the remedy becomes greater, and its effects more speedy.

It may be seen that I have drawn upon my notes at too great length, regarding a disease of only occasional occurrence, and the suggestions as to the treatment of which are rather negative than positive in kind. To this it may be replied, that we are wholly ignorant when, or how often, the epidemic may recur in its most severe form—that the long lapses of time without appearance of it are quite as extraordinary as its frequent return of late years—that the lesson of forbearance in some points of practice is as valuable as that of activity in others—and that the best chance of reaching further knowledge on the subject is to define exactly the limits of what we now possess. Any excuse that may still be wanting for the length of these remarks must be found in the singular character and history of the epidemic, and in its relation to other diseases; points to which I have already sufficiently adverted, and which render the topic a very interesting one to the medical inquirer.[1]

[1] A valuable volume has recently been published by the Sydenham Society, entitled "Annals of Influenza, or Epidemic Catarrhal Fever, in Great Britain," by Dr. Theophilus Thompson. This work contains the best attested records of all such epidemics from the year 1510 to the present time.

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CHAPTER XIII.

ON PROGNOSIS, AS A PART OF PRACTICE.

THERE are certain points connected with this important part of medical practice, which deserve more attention than is usually given to them. It is clearly most desirable that the rule of practice regarding prognosis should be as explicit as possible; compatible on the one side with our knowledge of disease, on the other with the conditions under which its treatment is conducted. The welfare of the patient is here not less concerned than the credit of the practitioner. As respects the latter, no better test can be had of his capacity and firmness than his conduct in this matter—the masculine ability to summon up at the moment the results of former experience, and to apply them soundly and discreetly to the case before him. The difference between the physician who has this capacity (whether original, or from cultivation) and him who has it not, is marked in every step of practice;—not less in the consistency of medical treatment, than in the uprightness of the moral relations, which need ever to be sustained towards the patient, and those who surround him.

Undoubtedly the basis of all rational prognosis is the true and thorough knowledge of disease; for sound conclusions can come from no other source; and reasoning and ingenuity are ever at fault, when wanting this foundation. He whose experience guides him to the best diagnosis is clearly most capable of foretelling the course and event of any given malady. All this may seem only the statement of obvious facts. There are, however, some considerations, belonging partly to the principle of prognosis, partly to the method and moral discipline connected with it, which need the careful regard of every practitioner, and especially of such as are young in their profession. That such need exists may be inferred from the various usage of physicians

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as to this matter;—a diversity arising often from differences of intellectual characters and habits; but, whencesoever derived, warranting the conclusion that there must be some course preferable to any other in this point of practice.

The ordinary duty of the physician as to prognosis is a double one;—that of forming justly his own opinion; and that of delivering it to others. The first belongs, more or less, to every case. In many, indeed, it is scarcely requisite that the judgment should take a formal shape, even to his own mind; but still, in all cases, the clearer his prospective view, the greater the security and steadiness of practice. The consideration how this judgment should be given to others, is a distinct, and, it may be, a subordinate point, but nevertheless one in which professional morality is concerned, and deserving therefore to be placed on a right footing, especially for those who are entering upon practice.

In neither one nor other point of view ought prognosis to be a hasty act. It is needful that the physician should be the more guarded in this matter, from the earnest solicitation to which he is ever exposed, tending in many ways to hurry and betray his judgment. The hasty opinion of an early moment often embarrasses every later part of his progress. It requires fortitude fairly to recall it, and to stand by the effects upon others of such revocation; while the attempt to evade this by subterfuge of words, is alike injurious to truth, and to the consistency and usefulness of practice. The caution which seeks to avoid these consequences is justified by their evil when incurred. And it is further warranted by the real difficulties which, even to the most experienced, attend the act of pronouncing on the progress and termination of disease. One physician may greatly excel another in the accuracy of this judgment—no one can be invariably right in it.

The question is one curious and instructive in many ways, as to the faculty of mind best fitted for forming a just prognosis in disease—and further as to the means by which this faculty may best be cultivated and enlarged. That the intellectual qualities here concerned are in great part special and original to each person, must be freely allowed. No education, though aided by large experience, can bestow that quick and almost intuitive perception, which seems to supersede reflection (*λόγου τι κρεῖττον*) yet which in reality depends upon reflection instantly and acutely applied. The judgment thus formed is drawn from ex-

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perience, equally with that reached by long laborious thought. But the difference lies in the faculty of directly evoking and applying it;—a power very different in different men by nature, but capable doubtless of being enlarged by effort and culture. The methods directed to this object are of singular importance in medical education;—not merely in the formal education which belongs to schools, but in that later and riper discipline to which a wise man will subject himself throughout every part of his professional course.

As a just diagnosis must ever be the most secure foundation of prognosis, the faculties upon which this depends especially require cultivation. It is well known to those who have purchased their experience at the bedside, that no mere book or lecture-learning can rightly convey this knowledge. Cases seen with the eye, and duly noted through all their stages, attest to the mind relations and distinctions of disease, which no other manner of study can afford. It may even be affirmed that the minute technicalities of diagnosis, as learnt by rote, are an incumbrance to the better judgment of the practitioner. Rules may be carried too far and made too specific, in a science, where the connections are so complex, the transitions of state so frequent and sudden, and where all practice must be submitted to so many exceptions and contingencies. Time and attention are sometimes flung away in seeking for an exact diagnosis, while symptoms are pressing upon us, explicit enough to mark the treatment required and the need of its being instantly adopted.

In diagnosis, indeed, as in every act of medical judgment, the demand for clear views, capable of being directly applied to practice, will suggest the value of a limited number of well-assured points, rather than a vague multitude of petty signs, the relative importance of which it is difficult to estimate or keep in mind. By such method we best define our knowledge, and render it of easiest and most speedy application; and this, where there is such proneness to error from the multitude of objects present, is sedulously to be sought for. The technicalities of names and nosological tables, needful though they are, and capable of being better perfected, unhappily afford only a partial and doubtful aid to the student seeking for sound knowledge. The number and diversity of such systems—each fulfilling certain objects, but none of them compassing all the conditions required

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—give proof of their present insufficiency as a foundation for medical diagnosis.

In forming our practical rule or method of prognosis, it is needful to admit many symptoms, as furnishing proof or strong presumption, which we cannot otherwise connect with the disorder present, than from our experience that they are generally indicative of certain changes or of a particular result. The picking of the bed-clothes as a sign of approaching death;—the similar inference from acid suddenly succeeding to alkaline urine in old affections of the bladder;—the deafness in fevers as a favorable augury;—these are simple instances of a class of prognostic symptoms familiar to every medical man, and often of material use in practice. That many of them seem arbitrary and anomalous is merely a declaration of our own ignorance as to the whole train of relations in each particular disease. Though perhaps less uniform in occurrence, and secondary or subordinate to other symptoms, yet are they equally natural as these, and standing in the series of causes and effects which make up the course of any given malady. Certain of these signs are common to many different diseases, inasmuch as they express a state of the whole or a part of the body, which these several diseases tend at a given time to create;—as, for example, certain peculiar conditions of the nervous or vascular system, the prostration of vital power, tendency to effusion, suppuration, &c. Such symptoms are often of much moment, not solely as prognostic signs, but as expounding the relations of disease, and furnishing or modifying the principle of treatment at certain stages of each malady.

It is worthy of note, how much more extensively the ancient physicians dealt with special prognostics than do those of our own time. Not having so fully methodized the history of disease, or illustrated it so largely by morbid anatomy and physiology, they naturally assigned more importance to those detached relations, which render particular symptoms the exponent of events to follow. The προρρήτικα and κρίσεις of Hippocrates are well worthy of study, in comparison with the results of modern observation on the same points. Some of them are groundless; others have local reference to the fevers of that coast and climate; but many will be found, well attested by experience, and applicable to the diseases of every country. The careful colla-

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tion of such signs might in various ways have value as a part of medical instruction.

One office of prognosis, too much neglected considering its practical uses, is that which relates to the effects of any given malady on the future health. The term of prognosis is not usually indeed thus applied; but the practical consideration, however designated, is one the physician ought never to lose sight of.

Maxims and means for the prevention of future disease come next to those for present cure; and the welfare of the patient requires that they should be inculcated in sequel to the latter. There are few maladies which do not furnish some such deductions; and in grave cases, especially, the duty of the physician does not wholly cease till he has drawn and enforced them, at the moment and in the manner best fitted to secure their adoption. Hinderance, indeed, often occurs in such cases from the indifference or ill-will of the patient towards prospective counsels, which interfere with settled habits or favorite propensities. Frequently, however, there is neglect of this matter on the part of the physician himself; and it is fit, therefore, that it should be inculcated as one of the determinate duties of his office.

I have already alluded to what may be deemed the moral part of prognosis;—that which makes the physician the interpreter of the events of disease to the patient and to those around him. This is a function often involving much that deeply concerns the success of medical treatment, from the very outset, and in every part of its progress. Nevertheless it is one upon which arbitrary maxims cannot easily or safely be laid down. Besides these conditions, scarcely to be numbered, which modify the aspect of each particular case, and render difficult any certain prediction of result, we have here to deal with the diversities of human character, and the various habits, fashions, and prejudices of society;—these themselves, it may be, affected at the time by the circumstances always present in sickness. No rules, definite enough to be useful, can cope with this diversity. There is but one general maxim safely admissible, and to which exceptions may not lightly be made. This rule is, that in any case where a deliberate opinion is required, and the fitness of giving such admitted, *truth should be maintained in all points*, as far as our real knowledge and conviction will warrant its declaration.

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It is the more needful to make this maxim explicit, since it is certain that a closer and more general adherence to it would not merely be compatible with the welfare of medical practice, but enhance its honor and usefulness. This is one of the many points where the prejudices and feebleness of the world react upon the mind of the physician, impairing oftentimes his firmness of purpose and conduct. In proportion as society becomes more artificial, this influence increases. The rule of avoiding what may give painful emotion, based on much that is natural and reasonable, acquires a force which usurps too far on truth, and fetters the expression of honest opinion. By a mental weakness, too, from which few men are wholly exempt, the phrases of its own creation return back upon the mind, and often greatly entangle and pervert the judgment.

These cases of perplexity are familiar to every medical man, and none, in truth, can altogether escape them. For it must be owned, and is admitted in the maxim just stated, that while truth is the supreme rule, there are cases where it cannot be fully or directly declared. The infirmity of human nature creates and vindicates this necessity. The exceptional condition includes chiefly those cases where evil would distinctly result to the patient from knowledge of the whole truth. In certain temperaments, and in particular maladies, the symptoms are liable to be aggravated by a sense or suspicion of danger; and confidence inspired becomes one of the chances and methods of cure. Or where positive cure is unattainable, as in certain organic diseases of the heart and brain, still the questions as to time of duration and comparative freedom from suffering remain open, and we cannot rightly incur risk as to these points by disclosures calculated to produce a sense of constant and imminent danger.

As I have already stated, however, no distinct line can be drawn, defining cases so infinitely varied in nature and degree. Even where the symptoms are seemingly the same, the mental temperament of the patient may be wholly different, and requiring to be differently dealt with by the physician. I think there is reason for affirming that the risk of evil from this cause is rated generally above the truth. In cases of imminent danger, the mind is not always, or even commonly, to be interpreted by the rule of health. Mental emotions are often altered in kind,

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or greatly abated in degree. Death itself, as I have had occasion to remark in a former chapter, is beheld under different views—a fact familiar to all who have watched over these scenes, and regarded the patient apart from those who are grieving around his death-bed.

However unlikely then it may seem, it is probably in cases of chronic disease, rather than of urgent danger, that exceptions, for the sake of the patient, are to be made to the rule before us. The consideration due to the feelings of those whose affections are centred on the patient is one of wider scope. There are cases, especially in long illness, where we cannot rightly refuse all hope (even though having it not ourselves) to those who are the daily and hourly ministers of aid and comfort, and whose power of being so is in part sustained by this feeling. Such instances, and others which arise out of the varying conditions of life, must be met as they occur, and the manner of doing this goes far to stamp the moral worth of the physician, including under this phrase as well firmness and uprightness of conduct, as those gentler feelings which carry consolation where it is most needed for the abatement of human sorrow.

With all admission of exceptions, however, the rule by which the medical man may best abide, is that of adherence to what he believes to be true; and I repeat my belief that the likelihood of evil or injury from this source is far less than common apprehension makes it. Suspicion of a painful truth often disturbs much more than the truth plainly stated; and glosses of speech generally end in impairing the confidence which it is so essential should be maintained. These considerations will not appear frivolous to those who rightly estimate the dignity of the medical profession. They are especially needful to the young practitioner, whose course in this stage is beset with difficulties, under which many give way; capable of being removed only by a principle of conduct, and by good faith and firmness in sustaining it.

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CHAPTER XIV.

ON PAIN, AS A SYMPTOM OF DISEASE.

THOUGH, strictly speaking, but an effect of disordered state or action of parts, and therefore mainly to be obviated by removal of the cause, yet is Pain, as a symptom of disease, a matter of great practical concernment to the physician and surgeon; and often needing consideration apart from the actual nature of the malady. We have to regard not only what it denotes, but what it produces by its own influence on the various conditions of physical and mental temperament. And while the topic is thus connected with a very interesting point of physiology, it blends itself in other ways with many important details of treatment, and even with what may be deemed the moral obligations of the medical man.

It is a just view of Aretæus, and well expressed, that "although it is impossible for the physician to restore health to all who are sick, to suppose which would place him above the Divinity, it is yet his lawful office to procure freedom from pain, and intermissions and suppression of disease."[1] The maxim that physical suffering should be obviated, whenever, and as far as possible, is indeed a sound one, both as respects the feelings of the patient, and the general success of medical treatment. Pain is occasionally the proof of beneficial changes taking place, but is never in itself a good. It always implies a deviation from the natural

[1] *Ὑγιειας μεν ἁπαντας ποιεειν αδυνατον τους νοσεοντας, η γαρ αν ιατρος χρεισσον θεου; απονιην δε και διαλειψιας και νουσων επικρυψιας δραν θεμις ιατρον.*

Boerhaave, in a chapter headed *Dolor*, in his *Praxis Medica*, expresses surprise that the subject of the alleviation of pain is nowhere to be found expressly treated of.

"Nil est in totâ medicinâ, quod Medico magis elogium procreare possit, quàm sopitio doloris; et tamen hæc materia nullibi tractata reperitur."

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state; and may therefore in every case be rightly regarded as an object requiring relief, where this can be attained without the sacrifice of other objects yet more essential. In numerous instances of common occurrence, it is, while present, an express obstacle to cure; hindering the right effect of remedies, and weakening those natural powers of the body, which resist disease.

The maxim thus generally stated, may seem too familiar to need assertion. But here, as in other points of practice, it is well to have a principle to which to refer;—subordinate, it may be, to others; yet blending with all, and often sufficing to turn the scale in doubtful questions of treatment. And further, there is reason to think that this principle is not duly inculcated as a precept in medical instruction. The description of diseases according to nosological systems, and of conventional methods of cure, too often supersede those more general rules, which apply to every point of practice, and are essential to its soundness and consistency.

The only qualification to this maxim is that suggested by Boerhaave; viz., that pain is beneficial as an index to parts in danger. "*Dico nullum esse meliorem vitæ custodem, nam docet partem in discrimine esse.*" Cases doubtless often occur, as I shall afterwards mention, where such knowledge, not otherwise easy of access, is thus obtained. But, in many other cases, the indications derived from this source are faulty or ambiguous;—and seeing the complex connections and sympathies of the nerves of sensation, and the great liability to error thence arising, we must receive under much limitation the conclusion just quoted. Even, indeed, were pain a more certain indication than it is, this would afford no reason against its relief, when such indications have been acquired. To allow its continuance as an exponent of parts affected, or of the severity of the disorder affecting them, would be a strange perversion of every sound principle of practice.

I am using the word *pain*, as the subject of this Chapter, in its most general and intelligible sense; and without reference to the many points in which physiology might be required for illustration. It is obvious even to common remark that pain is not a single or uniform state, but varying in kind not less than in degree, as far as simple perception can interpret to our

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knowledge. Excessive action in any nerve of sensation may be said to produce it; whether the excess be from sudden and violent impressions upon those nerves, or from action too long continued without repose. Here, then, in the outset, we have a source of great variety of painful sensations, each organic structure affording its particular class, and under every grade of intensity. To what extent diversity of structure may produce difference in the *mode* of pain, as well as in its intensity, is a curious question, not wholly solved by our present knowledge. It is difficult indeed to obtain any certain discrimination as to this matter. We possess no nomenclature—and from the nature of the case it would be hard to devise one—to designate either the kind or degree of suffering from morbid states of particular organs. Even the conventional phraseology of common life is at fault here. Every one has his own manner of describing bodily sensations; varying often in the same individuals; and widely different among the many whose lot it is to suffer;—a diversity which is sometimes the cause of considerable error in practice.

Take for illustration the pains and morbid sensations classed under the common name of *Headache*. How various are they in kind as well as intensity;—how certainly depending on different physical causes;—and how frequently requiring different and often opposite methods of treatment. Even the most experienced physician, though he may be able practically to relieve them, does so in frequent ignorance of the cause and seat of these morbid sensations. Or, again, how many and various feelings are comprised under the vague term of *pain in the stomach*—the effects of morbid action on, or within, those internal organs, which, variously susceptible themselves, produce by nervous connection a wide circle of sympathetic sensations throughout the body. Other such examples might be quoted; but they would lead too far from the subject before us.

The common assertion that one person bears pain better than another is doubtless just. This fact, in constant evidence before us, depends not alone on relative fortitude of mind; but also on a real difference in the nervous susceptibility of different individuals. Though the proofs do not reach certainty, they very closely approach to it. I have seen instances connected with surgical operations, which scarcely admitted of other solution

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than that of a physical disparity in different persons, and in the same individual at different times. It matters not whether we define this difference as belonging to the proper nervous structure, or to the sensorial recipient, of which the nerves are the instruments. The fact in question falls indeed under the more general law of the varying sensibility of all the senses; ever altering in the same person, according to the accidents of the body, and the relation between stimuli and the sensorial power; and never perhaps strictly alike in any two individuals of the species. The various phenomena of sleep, and of anæsthetic medicines, afford many of the most curious illustrations of the changes in sensibility of nerves, which may be effected both by natural and artificial means. Sleep in itself is more or less an anæsthetic condition of body; and in recognizing the innumerable gradations of state which actually intervene between simple drowsiness and the most profound coma (the only just manner of regarding this great phenomena of life) and the ever-changing degree of sensibility under these different states and stages, as well as from the many different ways in which sleep is produced—we have full evidence and illustration of the changes of susceptibility to pain which perpetually occur, in the same person, and even in what we term the waking state.[1]

If again, the inhalation of chloroform can utterly annul for a time all perception of pain, we have in this fact the index and proof of the numerous modifications, graduating between the normal state of feeling and such insensibility. The periods of youth and old age expound in more general manner this contrast of nervous condition in the same individual. It is very strikingly attested in certain morbid affections of body; as in the case of the painful sensibility of a palsied limb recovering its power, or in the morbid acuteness of the sense of touch in hysterical attacks; blending itself most curiously with the other bodily and mental conditions of this disorder.

The measure of diversity between one person or another is from obvious causes more uncertain, rendered so in part by the

[1] In my volume on Mental Physiology (Chapters V and VI), I have discussed fully this most curious and interesting topic; pointing out the relations of what I consider the true theory of sleep, to the phenomena of dreaming, madness, and intoxication; as well as to that particular phenomenon which has been called Mesmeric sleep, and which has so much perplexed the judgment of many thinking men.

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very different modes in which bodily suffering is expressed. But the fact of diversity remains as I have stated it; and the proofs are too common to need illustration. One of the simplest and most obvious is, the different effect of agents on the cutaneous nerves, where mental emotion is less concerned than in pain from wounds or disease of internal parts. To one person a blister is matter of little concern;—to another it causes extreme suffering. Instances of this kind might be endlessly multiplied.

Several points of practice connect themselves with the foregoing facts; but others much more important with a further general consideration regarding pain, viz., its effect in depressing or exhausting the nervous power. Such influence is unquestionable, nor perhaps can a single valid exception be stated to it. Without seeking here to define or describe this power, which is so deeply embodied in every part of animal physiology, it is enough for our present purpose to note the change induced upon it by bodily pain of any kind—this change varying in degree according to the amount of pain, its duration, and also, it may be, its localization in particular organs. The effect of exhaustion is sometimes disguised at first by the sort of struggle and excitement of the nervous system under suffering; but it is certain to show itself afterwards, and generally in proportion to such excitement. Illustrations of all this are equally familiar in common life, as in medical practice. Extreme pain gives a shock to the system, which in some cases and states of temperament, has been found actually fatal, without any other obvious functional change than sudden and complete loss of power. Deliquium often occurs in other cases, where the conditions are less urgent; under yet slighter degrees of pain, exhaustion is still proportionate to the suffering incurred. It is a further proof of the same fact, that where there is sudden cessation, or intermission, of acute pain, sleep frequently comes on instantaneously at every such interval of ease. The records of judicial torture furnish much striking evidence as to these effects; which are well known to us also in surgical operations, during the pains of parturition, &c.

Another collateral proof, and one very important in practice, is furnished by the experience of some of our best surgeons, that the successful issue of severe operations has been much greater since they began to use generally a more cordial regimen for patients operated upon. The benefit here is well testified by the

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more frequent prevention of erysipelas around the injured parts; and by the speedier repair of those vital powers depressed by pain, and needed for recovery. This alteration of treatment—justified as I believe it to be by its results—is not yet sufficiently embodied in general practice. In admitting the benefit thus obtained, we are bound to look to the certain mischief on the other side by adherence to the old and opposite principle of treatment. The practical interest and importance of the subject, as seen from this point of view, will readily be understood.

The relation of time to the effects of pain, and the power of enduring it, is a curious but doubtful question. Preternaturally strong impressions on the nerves of sense, or even such as are of natural kind, if long continued, undoubtedly impair their sensibility. But, on the other hand, the power of sensorial or mental resistance is certainly lessened or lost by the long continuance of pain; and we have examples constantly before us, where great or even fatal injury is produced by the persistence of suffering which, in the same degree and from the same causes, has been readily borne for a shorter period of time. These in fact are cases of nervous exhaustion, gradually augmenting from the continued operation of the cause.

Reverting now to the principle before stated, that pain is always an evil, the abatement of which is to be sought for, whenever the means are not incompatible with other more important objects of treatment, we come at once to the question regarding the fitness of remedies directly adapted to this object. The case of pain from external injury by wounds, burns, or operations, occurs here as the simplest condition of the inquiry. That from a severe operation is a shock to the nervous system of the nature above described; often so obvious and urgent, that no practitioner will hesitate in seeking to give the assuagement required. There are in truth few cases of this kind, where the relief of pain may not be made a direct object in treatment; thereby assisting the constitution to sustain the injury inflicted; and giving effect to other means which could not else be efficiently used.

When I first wrote this chapter, now several years ago, the use of Chloroform and Ether as agents for annulling pain, was, except by some vague anticipations, utterly unknown to the world. The discovery and application of these anæsthetic powers forms an era in medical science and practice. I need not dwell on a

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subject now so familiar in all its details; or do more than express my opinion, that no controversy ought longer to exist as to the nature and amount of the remedial good thus placed in our hands. Exceptional cases exist—fatal in event, and often fearful in the manner of it,—but still they are exceptional. If they seemingly multiply in number, it is because the use of chloroform has become much more universal; and because a more careful record is kept of anomalous results in the case of a new remedy, than of those which have long been familiar to us. Time and experience will probably lessen the proportion of these unhappy instances, by giving us more exact knowledge of the cases where the condition of the patient precludes the use of the remedy; and thus rendering its manner of use more certain and safe. But even as it now stands, we have an agent in our possession, powerful for immediate good, and capable, doubtless, of more various and modified application than our present experience has given to it.[1]

I do not here speak of the curious physiological questions, mental as well as bodily, connected with the theory of these anæsthetic agents. Our concern in this place is with their practical application; and in relation to this it will be obvious that it is chiefly in the cases of sudden, severe, and transient pain, already alluded to, that their employment is of greatest avail, and especially warranted. In other parts of practice, where the relief of pain is our object, Opium must be looked to, as heretofore, as the most efficient remedy we possess; capable, under right employment, of meeting most of the contingencies for which we apply to it. In another chapter (On the Use of the Opiates), I shall speak of the causes which have produced a needless timidity in regard to this great medicine, interfering with its real efficacy in the treatment of disease; and shall state what I consider the principles by which its employment as a remedy may

[1] It can hardly be doubted, that time will increase the number of these anæsthetic agents in our hands, and not impossibly with new modifications of their manner of influence. It must be recollected that the sleep or stupor, which ensues upon intoxication, is an example of the effect in question, as indicated by many familiar facts.

The recent suggestion of cold, as a topical anæsthetic agent, is warranted already by a certain amount of successful experience, and merits further trial. It is a curious instance of the same result obtained by means totally different in principle and manner of action.

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best be guided. In reference to its use for the express relief of pain, I may mention by anticipation one fact, equally curious and important, viz., that opium given for this object, if effecting it, does so without producing (or at least produces in far less degree) those other effects on the nervous system and natural functions, which follow its action, when not thus called for. This fact (which has its analogy in the case of some other medicinal agents) is by no means adequately regarded in practice; seeing that it often sanctions a freer use of the remedy than is actually made, and better defines its manner and limit of employment. If justified in bringing opium to the relief of any given pain, the same condition justifies us in using it boldly and sufficiently;—that is, not falling short of the attainment of relief. Sleep, always prone to follow the cessation of severe suffering, is the fairest result and test of the completeness of the remedy. But whether this occurs or not, the general rule holds good, that the best measure of opium in such cases of employment, and the best security against ill effect of any kind, is its sufficiency for adequate relief.

It may be added, in relation to the same view, that a single full dose is often of greater avail than the same quantity divided, and given with intervals between. The specific impression on the nervous system is greater, as well as more speedily attained. There are many cases where this rule requires to be modified by the necessity for intermediate observation, or other causes; but the general view is one which it is well to hold in mind in practice.

The question as to the fitness and manner of relieving pain in disorders of internal organs, where no obvious cause of lesion is present, is one of much more doubtful kind, and less capable of being solved on any general view. Pain is here especially one of the symptoms expressing the malady, pointing out its seat, and often furnishing evidence as to its degree. For obvious reasons, this remark particularly applies to those cases where inflammation is present. As it is well known to us that inflammatory action in any tissue may go on, though its sensibility be for a time artificially lessened or removed, it is a maxim recognized by prudent physicians, that such diseased action should not be hastily or heedlessly masked by mere anodynes; at the expense of remedies better befitting the symptoms, and with the loss of

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the time when they may most beneficially be used. It is needless to cite particular instances in support of this rule. Medical practice furnishes them every day; and especially in inflammatory affections of the brain, of the heart and lungs, and of the alimentary canal.

Nevertheless, in these cases also, the principle of relief to pain is still to be kept in mind; modified and guided by a higher expediency, but not abandoned. Amidst antiphlogistic means, or in conjunction with them, regard may still be had to this object; and very especially as it concerns the procuring of rest at night. The absence of sleep (*φιλον θελγητρον, επικουρον νοσου*) disturbs and impairs more or less every function of the body, and adds thereby to the ills already suffered. Its restoration, if not attained at too great expense in other ways, tends so greatly to equalize the circulation, subdue irritability, and correct disordered actions, that it must ever be kept in view, even in the cases just cited. Yet further, the exhaustion of the vital powers by pain can rarely be affirmed to give relief to local inflammation; while, by bringing in fresh causes of disturbance, it often interferes with the means appropriate to the malady, and renders eventual cure less speedy and complete.

Experience concurs with theory in the distinction between inflammatory and spasmodic disorders, as respects the treatment available for the relief of pain. This consideration is especially important in disorders of the stomach and intestines. Colic pains from irritation of the bowels,—whether produced by flatus, disordered secretions, or acrid matters in the canal;—call for relief by direct removal of the cause, if it be possible; but very commonly require some combination of anodyne with the purgative employed to effect this. Opium is a remedy with which we can scarcely dispense in the treatment of spasmodic irritation of these parts. It often removes what forms the impediment to natural action, and thereby facilitates the operation of other means of relief; while the pain resulting from drastic remedies is generally in itself a notable aggravation of evil;—more frequently, it may well be affirmed, than enters into the calculation of common practice. The just discrimination in cases of this kind, is that which forms the distinction between the good and the bad practitioner. It often, in truth,

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decides the question between the good or bad result of the malady.

The fitness of relieving pain in neuralgic affections, properly so termed, need hardly be inculcated, as this is assumed, and acted upon, in all ordinary practice. Only one qualification need be stated, viz., that the relief of present pain in such cases is not the removal of the cause of it. We are for the most part obliged to look elsewhere, and to other means, for the attainment of this object. But it is true here, as in instances already cited, that the presence of pain does really often retard cure by creating other disturbances in the system. And further, it may be remarked, that in cases where the pain assumes a periodical character, the suspension of a paroxysm, by breaking into the series of morbid actions, will oftentimes aid and accelerate the cure of the disease.

The question is more ambiguous as to the means we may fitly employ for the abatement of pain in diseases of membranous or articular inflammation, as gout, rheumatic inflammation of the joints, &c. It must be kept in mind here also, that merely to assuage suffering, is not to remove its cause; but that in giving such relief by means which do not infringe upon other objects of treatment, we often facilitate the cure, and better preserve the constitution during its progress. Many old questions of practice occur here; hardly yet solved, though often present to us for solution. They may all, as I believe, be submitted to the principle that both gout and rheumatic fever depend on states of the blood, rendered morbid by the nature or excess of certain ingredients circulating in it; and that a particular train of actions is necessary to the change of such matters, or to their elimination from the system. These actions, whether with or without the interference of medicine, mainly constitute what is called the disease;—analogous in this to the series of symptoms which make up the history of measles or small-pox, though much more irregular than these diseases in course and aspect. I do not press this as a new mode of viewing the subject, but merely as one not sufficiently regarded in practice.

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CHAPTER XV.

ON POINTS WHERE A PATIENT MAY JUDGE FOR HIMSELF.

WHAT are the circumstances and conditions in which the suggestions of the patient himself may safely, or expediently, be admitted in the treatment of disease? Every prudent physician will make himself aware of these, in as far as they can be reduced to anything like rule; and guide his practice more or less closely by them.

First.—The patient may almost always safely choose a temperature for himself; and inconvenience in most cases, positive harm in many, will be the effect of opposing that which he desires. His feeling here is rarely, if ever, that of theory; though too often contradicted by what is merely such. It represents in him a definite state of the body, in which the alteration of temperature desired is that best adapted for relief, and the test of its fitness usually found in the advantage resulting from the change. This rule may be taken as applicable to all fevers, even to those of the exanthematous kind; where, with an eruption on the skin, the balance between the outer and inner surfaces of the body, and the risk of repression, might seem, and actually are, of greatest importance. In whatever stage the eruption be, if the patient expressly seeks for a cooler atmosphere or cooling applications, they may be fully conceded to him without fear of ill result; and under the guidance chiefly of his feelings as to the time during which their use may be continued. Except in some cases of vitiated sensation from nervous disease, I have scarcely ever known the judgment of a patient practically wrong on these points; and in this case of exception the error itself is of very little consequence. Common practice is here so much wedded to old prejudices that it is well to have something like a principle to which to refer for direction.

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Secondly.—In the majority of instances of actual illness, provided the real feelings of the patient can be ascertained, his desires as to food and drink may safely be complied with. Whatever be the physical causes of the relation (and they are yet beyond our research), the stomach itself is the best expounder of the general and more urgent wants of the system in this particular. But undoubtedly much care is needful that we be not deceived as to the state of the appetites, by what is merely habit or long impression on the part of the patient, or the effect of the solicitation of others. This class of sensations (instinctive in its origin) is much more nurtured out of the course of nature than are those which relate to the temperature of the body. The mind too becomes much more deeply engaged with them; and though in acute illness they are generally submitted again to the natural law, there are many lesser cases where enough remains of the leaven of habit to render every precaution needful. With such precautions, however, which every physician who can take schooling from experience will employ, the stomach of the patient becomes a valuable guide;—whether it dictate abstinence from or recurrence to food,—whether much or little in quantity,—whether what is solid or liquid,—whether much drink or little,—whether things warm or cold,—whether sweet, acid, or saline,—whether bland or stimulating to the taste.

There may seem some exception to be made for those cases, where urgent thirst gives the wish for liquids of a kind hurtful to the stomach. But it is the fluid alone which is the object of desire; and when the choice is before the patient at the moment, he will usually take that which most simply satisfies this natural want. Attention to this point is especially needful in the treatment of infants.

As respects limitation of food, the "*tempestiva abstinentia*" is generally with the patient himself an urgent suggestion of nature; especially in the many cases where fever is present, or where the digestive organs are the seat of disorder. It is a part of the provision for cure which is even forced upon our attention; and if not sufficiently regarded, all other remedies lose greatly of their value. Here, then, we are called upon to maintain the cause of the patient, for such it truly is, against the mistaken importunities which often surround him, and which it requires much firmness in the physician to put aside.

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It is not wholly paradoxical to say that we are authorized to give greatest heed to the stomach, when it suggests some seeming extravagance of diet. It may be that this is a mere depravation of the sense of taste; but frequently it expresses an actual need of the stomach, either in aid of its own functions; or indirectly, under the mysterious law just referred to, for the effecting of changes in the whole mass of blood. It is a good practical rule in such cases to withhold assent, till we find, after a certain lapse of time, that the same desire continues or strongly recurs; in which case it may generally be taken as an index of the fitness of the food desired for the actual state of the organs. In the early stage of recovery from gastric fevers, I recollect many curious instances of such contrariety to all common rule being acquiesced in, with great alarm to those around the sick-bed, but with manifest good to the patient. Dietetics must become a much more exact branch of knowledge than is now the case, before we can be justified in opposing its maxims to the natural and repeated suggestions of the stomach, in state either of health or disease.[1]

These remarks (a repetition, in part, of what I have said in a former chapter) are, I am aware, liable to cavil in various ways. But restricting them, as I do at present, to the case of actual illness, and even then rather as a guidance than a positive rule, I believe they will be found not merely admissible, but useful, in general practice.

Thirdly.—As regards exertions of body, posture, continuance in bed or otherwise, the sick may generally be allowed their own judgment, provided it is seen to be one dependent on bodily feelings alone. And so equally with respect to fresh air, methods of exercise, and times of repose. In these things, as on points of diet, suggestions, founded on careful notice of the feelings of the patient, and watchfulness as to the effect of the first trials, are all that is required from the physician; and more than this often does mischief. I have frequently witnessed

[1] Aretæus says judiciously, in a passage which applies to all parts of this subject, *Εστω τοισι καμνουσι χαριτες, και το καθαρως άπαν δρᾶν, και τοισι επιθυμιησι ἑπεσθαι τον ιατρον μη μεγαλα βλαπτοντα· αριστον γαρ τοδε, ην μη καρτα ωφελουντον ορεξις ἡχῃ.* And we have an earlier and still more special maxim to the same effect in one of the aphorisms of Hippocrates. *Γο σμικρῳ χεῖρον και πομα και σιτιον· ἡδιον δε, των βελτιονων μεν, αηδεστερων δε, μαλλον αἱρετεον.*

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the ill effects of minute interference in such matters; whether arising from excess of caution, or from the mischievous spirit of governing everything by medical rule and authority; without appeal to the feelings of the patient, even where these may most securely be taken in evidence. Guided by these feelings, I have often been led to sanction the journey of a convalescent at so early a period, as to alarm all those who had been anxiously watching over the illness. I do not recollect a single case in which I have had cause to repent this acquiescence.

The most important exception to this rule is in certain nervous and dyspeptic disorders of chronic kind, where it is needful to urge bodily exertion upon the patient; in contradiction to his own sensations, and sometimes even where the first trials are seemingly unsuccessful. With moderate care in observation, the tests of fitness here are so simple, that there can be little chance of any error leading to injurious consequences.

As respects mental exertion during illness or convalescence, much more caution is needful. Here the patient is usually less able to estimate his own power, and is more entirely at the discretion of those around him. The present condition of life among the higher classes produces as much of evil from excesses of moral and intellectual excitement, as from those of the stomach: and it is equally difficult to place watch and reasonable restraint upon them. In these instances, and they are of constant occurrence, the judgment of the physician, as well as firmness in his manner of interference, are urgently required. But in ordinary cases, and under more tranquil methods of life, he may leave much to the discretion and feeling of power in the patient himself; with simple injunction that this feeling should be duly consulted before any change is made.

Fourthly.—The prudent physician, without sacrificing his science or authority, may often draw knowledge from his patient as to the fitness of particular medicines, or the fitness of employing medicines at all. Frequently it happens that the latter, at some period or other of his disorder (usually when convalescence has begun), expresses strongly his feeling that medicine will do no further good, and that his future progress is best left to nature alone. Provided it be clearly seen that this does not arise from any whim or perverseness of mind (and the feeling may exist wholly without such), the medical man is bound to give attention

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to it, as suggestive at least of his further treatment. And if continued and repeated, at a time when the urgent symptoms of disorder are past, it may for the most part be rightly acquiesced in;—certainly so far as to make trial of the effects of the change; which, being of negative kind, can rarely produce much evil, even if it fail of good. Convalescence is a wholesome process of nature, not wantonly to be interfered with by any less certain resources of art.

Looking fairly, indeed, at the common course of practice in this country, I believe much more mischief to be done by needless protraction of the use of medicines, than by the premature abandonment of them;—a point if true, requiring attention to all such means as may best contribute to its reform, seeing the various injury thus inflicted upon all the objects and higher interests of medicine. The case just stated is one of those where the practitioner may often effect this, in the way most conducive to the welfare of his patient; and it is his duty not to disregard the suggestions so obtained.

There are other instances, as mentioned above, where the persevering distaste to a particular medicine, or method of treatment, will not only sanction, but call for its disuse; though the authority of all ordinary experience be in its favor. On points of this kind a discreet forbearance is as needful to the physician as firmness. And the best rule is, not to be implicitly subservient to rule:—a maxim essential in all cases to the right practice of medicine, seeing the many contingencies which are ever arising in contradiction to its soundest methods and precepts.

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CHAPTER XVI.

ON METHODS OF PRESCRIPTION.

The simplification of medicine, and of methods as well as principles in the treatment of disease, ought ever to be present to the mind of the physician. The more complex the objects before him, the stronger the motive for adopting and maintaining this rule. Even with all the curtailments and sounder views of modern practice, it must be admitted that more might yet be done in furtherance of the object. We have long abandoned those preposterous compounds, of which the number of ingredients, or the secret methods of admixture, formed the sole science; but the error is still frequent of aiming at a greater number of objects than can be reached at once, or by any single combination of remedies; thereby forfeiting the good to be derived from simpler and more explicit means. Secondary and subordinate symptoms are apt to usurp a place in prescriptions, due to such only as are essential to the character and course of the malady. And it is not too much to affirm that the judgment of the medical man upon a disorder is often warped by the reflection of his own practice. He is apt to look at the symptoms through his own previous treatment of them, as well as through the false opinions of those around; and the trifling or casual symptom of one day gains undue weight the next, from its needless admission among the objects for which he has prescribed. All this is natural in itself; but undoubtedly a serious error in practice. In no way can it better be obviated than by carefully simplifying the character of prescriptions; and avoiding ambiguity, as far as possible, in the intention we assign to their several parts.

It is an opinion, not infrequent among medical men, that the multiplication of medicines in our hands adds in the same ratio

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to the power and facilities of practice. This view is admissible only in a very qualified sense. It is, in truth, as correct to say, that the addition of new medicines or preparations, which do not expressly accomplish new purposes, or fulfil more advantageously those already attained, becomes an incumbrance to the practitioner, and an impediment to the progress of the science.

It may be alleged, and must be fully admitted, that combinations have often effects not resulting equally from any of their ingredients. This is true as respects many vegetable medicines of powerful action, whether narcotic, purgative, diuretic, or alterative. It is equally true as regards chemical medicines, where changes of combination may occasionally be calculated upon, favoring the effects desired; though much more frequently the facts by which their use is determined are merely empirical in kind. The observation of these facts is obviously one of the most important objects to the practitioner. But it must be added, one of the most difficult also; for even in the simpler combination we can rarely obtain that precise estimate of effects, which is so essential to the success and certainty of practice;—still less can we do so in those of complicated kind. Each new ingredient added to a medicine raises to a higher ratio the chances of error, and obscures the evidence by which such error may be detected and removed. And the application of other science to this subject (though never to be lost sight of) is here made so uncertain by being subjected to vital actions, that it must ever be admitted with great caution, and wholly in subordination to experience. Several compound medicines, of undoubted efficacy, contradict chemical laws even in the points where it might seem of greatest moment to maintain them.

The best general rule seems to be, to define to ourselves in every case, as distinctly as possible, the main object of present treatment—to make this the basis of prescription—and to admit, subordinately and under limitation only, all such means as apply to collateral symptoms. This rule ought especially to be kept in mind, when a new medicine, or new applications of an old one, are on trial in our hands. Complexity here, especially that which arises from the admixture of new agents in the same prescription, disguises those results which it imports us most to know, and which are always learnt with difficulty amid the many conditions tending to obscure the effect. I have often

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seen prescriptions in which three or four powerful agents, still new to our experience, were put down for simultaneous use, in such manner that no interpretation could possibly be obtained of their respective influence on the disease. In adding to other difficulties the uncertainty of combination, which is itself in most cases an experiment, we wrap one doubt within another, and play false with every just principle of medical research.

Though it may seem inconsistent with the timidity of early practice, yet do we find error as to these several points most frequent among those still young in their profession. Some run after new medicines from the genuine desire of discovering new facts;—others from the more meagre motive of not appearing behind the knowledge or fashion of the day. The young physician, moreover, is often led to a needless or injurious complexity of prescription, in his eagerness to meet each single symptom by some specific antidote; without due regard to those general characters of disease upon which all particular symptoms depend. The *nimia diligentia* is sometimes in this case more hurtful than positive neglect.

Some of these recommendations may seem trite or trivial; yet are they justified by the rarity of a principle of prescription, consistent with itself, and giving due proportion to the objects in view. The fitness of a man's understanding for medical practice can in no way be better estimated than by looking to these points. The physician who allows his attention to be distracted by secondary symptoms, and hampers a powerful medicine with petty appendages, directed to subordinate objects, shows a quality of mind adverse to sound practice, and incapable of attaining any valuable truth.

Not in the technical part of prescription alone, but also in the examination of symptoms on which it is founded, much may be gained by a *rule of inquiry*. Vague and inconsecutive questions, whether from timidity in the young practitioner, indifference in the older one, or irregular habits of thought in either, are very generally useless, and often positively hurtful. They defraud the physician of his own judgment, and put the mind of the patient into the state least apt to give right information. In the adoption of particular methods to obviate this, every one having discretion should exercise his own choice. A man of sound understanding, and who holds good faith with himself, may dis-

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cover (and it is fit he should seek to do so) in what points his faculties are oftenest at fault; and this ascertained, he is in case to determine how the correction may best be made.

Rightly used, and not allowed to become too technical, a certain rule pursued in questioning the sick will strengthen, by disciplining, the powers of observation. Nor is there much danger lest this should cramp that higher and rarer quality (derived either from long experience, or from quick natural perception) which enables some men—the *abnormes sapientes*—to comprehend at the first glance, to seize by a sort of intuition, all that is most needful to be noted in the diseases before them. These instances are not so frequent as to form a rule; nor can the faculty, though an eminent one in itself, ever entirely preclude the necessity of further research. The persons thus gifted, moreover, cannot really be fettered by methods; and will use them only so far as they are actually conducive to good.

It is a frequent error in young practitioners to allow themselves to be betrayed into hasty diagnosis or prognosis of disease, either by their own nervousness, or by the importunity of the patient and those around him. The fault is serious;—not merely as a source of embarrassment, but often by impairing integrity in practice, from the desire to redeem a wrong opinion thus given. It concerns greatly the reputation and usefulness of the profession, that every caution should be exercised on this point. Forbearance in words, till the judgment is well decided, is therefore in some sort a duty, as well as an act of prudence. And it further behooves the physician to acquire as much mastery and readiness as possible in the observation of those symptoms which distinguish a disease or foretell its probable event;—another phrase, it may be, for medical experience; yet with the advantage of being more explicit as to a very important part of practice, of which I have had occasion to speak at greater length in a preceding chapter. If mistakes in judgment be still made (and this will occur to every one), though their avowal is not always needful or desirable, yet I believe the character of the profession, as well as of the practitioner, to be best sustained by truth;—spoken as a man of sense and integrity will speak it. This is a point of medical morality to be impressed upon all who are still young in their professional career.

Not only in the examination of symptoms, but also in describ-

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ing them, it were much to be wished that something like a determinate method could be devised, admitting of being carried into general use. Such method, if attained, would render practice itself more exact, and the record of cases more intelligible and useful to others; besides facilitating greatly the study of those general relations of disease and remedial means, upon which the progress of medicine, as a science, mainly depends.

In the treatment of disease, it is a convenient rule to look first to the *external remedies* befitting each case, before determining those for *internal use*. This also may seem a trifling suggestion. Yet such technical arrangement, if involving no practical error, is generally useful, and especially to the young practitioner; who is often painfully harassed, not only by the responsibility of instant judgment, but by the number of objects and methods present to him for consideration. Nor is it the technical aid alone which is of value here. It is well that every practitioner should keep in mind the expediency of attaining all that is possible by external remedies;—a discreet preference, strongly sanctioned by modern research, and in no wise incompatible with the bold and sufficient use of internal means when called for by the more urgent necessities of practice. Such suggestion is the more needful, as remedies directed to the skin are those most apt to be neglected altogether, or carelessly and inadequately employed. The draught and pill are taken with strict regard to time and order. The outward application is forgotten, or hurried over as a troublesome appendage to other treatment. As in argument some men's minds are indisposed to be content with common reasons, however cogent, so here the obvious and simple nature of the remedy becomes a hindrance to its fair and sufficient adoption.

The physician who leaves the bed-room of his patient, especially in cases of fever or acute disorder, without attending to more than the prescription of medicine and diet, has very imperfectly fulfilled his office. He is bound further to look to temperature and ventilation; the fit state of the patient's bed; his posture; the needful changes of clothing; the proper use of water for cleanliness or coolness; and the maintenance of quiet. These things, contributing alike to the comfort of the patient and to the chances of recovery from disease, are often, it must be admitted, passed over, or too hastily dealt with, in the hurry of practice;—an omission the more important, as many of them are expressly

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the subjects of popular prejudice and mischievous error. The patient himself being often wholly unable to explain what is needful to his situation, the physician is bound to act for him in regulating the whole economy of the sick room. A simple change of position, of bed-clothes, of the air of the chamber, will often do more towards the relief of suffering, than a page covered with the best medical Latinity.

The ability and good faith of the practitioner are, in fact, equally put to test in these less ostentatious parts of his science, as in those to which common opinion assigns a higher value. And it is a further reason for the use of certain regulated methods in these details, that nothing may be omitted, where so much is to be considered and done; and this under circumstances the least favorable to collectedness of thought, or exactness and sufficiency of direction.

There is yet another point connected with this subject which I cannot forbear noticing, however slightly. This is the influence of the countenance, the manner, and language of the physician on the feelings, and, through them, on the bodily welfare of the patient. It needs no learned reasoning on the mutual influence of mind and body, to enforce this precept. The experience of every man, and of every day of life, denotes its truth; and medical experience more especially has the fact ever before it. A few simple and soothing words of comfort and hope, when they can justifiably be spoken to the patient, not merely dispel the gloom of a sick bed, but in many cases effect a positive good which no medicine could equally procure. Here, then, is a practical duty of the physician;—difficult of exercise, it may be, where dangerous disease is present; yet ever to be kept in mind as of those happier remedial means which are committed to him for the welfare of others; and not without a reflected good upon his own character and feelings.[1]

[1] With his wonted good sense and sound experience, Celsus inculcates these duties of the physician in a sick room; "*assidere hilari vultu:*" . . "*probabili sermone lenire,*" &c. The medical scholar will not fail to recollect the various precepts which have reached us under the name of Hippocrates, as to the methods fitting to be observed in communication with patients; together with his admirable maxims touching the moral demeanor of the physician in his professional life.

If it should seem that the remarks in this chapter are either too familiar, or made too much *ex cathedrâ*, I must plead my conviction from experience of their importance; and my hope that they may be useful to some who need guidance as to such details, in the earlier and more difficult stages of practice.

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CHAPTER XVII.

ON INTERNAL HEMORRHAGES AND MORBID SECRETIONS AS THE SUBJECT OF MEDICAL TREATMENT.

In several other places I have had occasion to allude, directly or indirectly, to this topic; one of the most important in medical practice, and liable to much difference of opinion upon points where wrong views may do, and have done, very great mischief. If it seem objectionable to comprise so much under one title, the answer must be found in the expediency of reaching some common principle in this part of the treatment of disease;—a principle necessarily, indeed, very vague in many of its applications; yet less so than the practice usually pursued, and ever pointing towards a better rule in cases which are peculiarly the subject of rash and fluctuating judgment.

The practical question which stands foremost in each one of these cases is,—whether any given secretion or excretion, morbid either from excess of quantity or alteration of quality, is to be suppressed or otherwise altered by medical means, if this be attainable?—or whether such morbid appearance is to be received as producing or expressing some change in progress, salutary to the body, and therefore not to be checked or interfered with? There may be other modes of stating the inquiry; but practically they all come to the same point. And every physician will recognize in his experience the numerous cases in which this question supersedes all others, and cannot be evaded without endangering the results of his treatment.

It is in truth a branch of that more general question as to a *vis medicatrix naturæ*, which has exercised the thought and speculation of physicians from the earliest time, and yet remains to us a perpetual problem in practice.[1] The phrase may be too

[1] The expressions of Hippocrates (Epidem. VI, 5, 1), *Νουσων φυσεις ιατροι· ανευρισκει ἡ φυσις αυτη ἑαυτῃ τας εφοδους*, have given text to argument and diversity of opinion from his time downwards to our own.

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general for the foundation of a system; yet is it justified as involving an important reality, which if kept constantly present before us, will be found to illustrate pathology more extensively than any other; and to contribute more largely and effectually to the success of medical treatment. The distinction between diseased and curative actions is at the root of all rational practice; and no medical man can expediently or even safely disregard it.

In discussing that part of the subject which the title of this chapter includes, I shall notice those instances especially which are either of greatest practical moment, or best illustrate the principle conveyed in the phrase just stated.

The subject of hemorrhages, as viewed under this light, is that which has perhaps received least attention, in proportion to its practical importance, though from various causes most exposed to the chances of error. Hemorrhages from vessels accidentally wounded, or intentionally opened, may be excluded from the inquiry. But all such as occur within the body from structural changes; from irregularities in the flow and distribution of blood; or from alterations in its quantity or quality, come fairly within the scope of the question. No practice can be sound or salutary which does not keep this question in view. Is hemorrhage from the vessels of the lungs, or from those of the stomach, intestines, or kidneys, to be directly repressed by any such means as we possess? Or are we to give a certain scope and license to what is for the most part but an effect of some morbid cause in operation, and often capable in itself of obviating the evil which produces it?

Hæmoptysis may furnish the first instance in illustration. We have here blood coming from some part of the lungs—an alarming evidence, in most cases, of a state of structure verging on disease, and that of dangerous kind. The symptom justifies the fear with which it is regarded; but not the interpretation that the hæmoptysis is an integral part of the disease; or urgently to be restrained, because we desire to obviate the morbid condition producing it. It is seldom that such quantity of blood is lost in this way as to be in itself any just cause of alarm. As much is often taken away by bleeding, under the profession of checking this internal hemorrhage—a treatment very doubtful in principle, and, according to my experience, little to be relied on in effect. The action of the heart may be so far subdued

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that the internal bleeding ceases; but the blood is lost in another way; the disease which produces the symptom remains unchanged; and the power of resistance to its future progress is often injuriously lessened by the supposed remedy.

But, further, is it certain that, in hastily checking Hæmoptysis, we are not repressing what is really a remedial discharge? Is not this internal bleeding, when moderate in degree, often the best means of relieving that local inflammatory state, which either results from disease already in progress, or is concerned in producing it? The capillaries unduly loaded, in parts and textures to which we have no direct access, suffice thus for their own relief; the effect abating when this pressure upon them is lessened or removed. Or if the expression of *remedial* be too strong for this case, may we not still fairly ask from experience what are the evils arising from hemorrhage within the lungs, except where much disease with debility already exists, or the bleeding comes from a vessel of such size that no means can be found to control it? In other cases, even those of incipient tubercular disease, alarm is much less justified by the loss of blood, than by the prospect of the fatal disease of which it is the precursor and omen.

Hemorrhage from the alimentary canal,—whether Hæmatemesis, or the discharge of blood from the bowels—affords instances upon which we may still more securely rely. Some of these cases are clearly vicarious, having relation to the menstrual discharge in women; and thus far carry with them the proof that they are not insalutary in kind. But the same conclusion may be drawn as to many cases, not thus clearly designated, from the effects of the hemorrhage, either immediate or indirect; —these results being often so clear as to allow no other interpretation. Every practitioner must have been struck with the fact, in certain cases of blood vomited from the stomach without obvious cause;—often exceedingly profuse in quantity, and in quality such as to show that the venous system of the abdominal viscera has largely emptied itself through this channel. It cannot be denied that there is mischief from hemorrhage in some of these cases; as the exhaustion is occasionally great, and needs care and time to repair it. But if it occurs by simple exudation, and without any *diseased texture* concerned as the cause, it is rarely, if ever, fatal in event;—not often hurtful, except from

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debility;—and generally, it may be added, sufficient for its own relief, in the very depletion of vessels thus produced. And what is more to our purpose, it may be affirmed that such hemorrhage does in numerous cases relieve the system from causes of actual or contingent disorder, which no medical treatment is capable of removing with equal security, speed, and completeness. Where we cannot show the discharge to be a vicarious one, we still obtain proof of the results just stated, from comparison of the state which follows the hemorrhage with that preceding it.

It is highly probable, in some of these cases of relief to the system, that the *quality* of the blood, thus drained from the vessels, is concerned, as well as its mere quantity, in producing the hemorrhage; and that the benefit depends chiefly on this circumstance. Without entering into the questions, still not settled, what different states the blood may assume in different parts of the venous system, and particularly in the great venous trunks associated with the portal circulation, we have proof enough to be assured that these conditions are very various;—that the blood undergoes great changes in quality in these vessels, some of them noxious in kind, altering the glandular secretions, and sometimes forcing on hemorrhage as a result. It may not be easy, and is often impossible, to decide as to these points in particular cases; but in every such case there is reason enough to enforce the question, whether we are to aim at checking the hemorrhage by direct and cogent means, or to seek only to moderate excess in its degree, by withdrawing all that may directly excite or protract it?

A sound practice will, I believe, sanction the latter treatment in the majority of instances. If care be given not to exasperate the hemorrhage by any excitement of the vascular system;—if quiet, abstinence, cool drinks, and counter-irritation to the abdomen, be duly employed;—the true indications of treatment are generally better fulfilled than by venesection, metallic salts, mineral acids, astringent medicines, and the other means with which an officious practice hurries to the relief of these cases. I do not affirm that this negative treatment is to be applied in all instances, or in all stages of such hemorrhage; but in principle it is assuredly that which is safest and best; and the exceptions may usually be suggested and provided for by the discretion of the physician at the time.

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Hemorrhage from the bowels (excluding that incidental to Hæmatemesis) may probably proceed from any part of the canal; but occurs more especially in the colon and rectum, in connection with hemorrhoidal swellings, enlarged veins, or other causes of local irritation in the bowel. The bleeding, which forms so frequent a symptom of piles, is important to the general view before us, as singularly testifying in many cases its connection with the state of other parts; and through the portal circulation especially, with some of the greater organs and functions of the body. The very obvious relief, in effect of natural bleeding from the hemorrhoidal vessels, to disordered states of the head, of the heart, of the liver, &c.; and the occurrence or return of such disorders, when an habitual discharge of this kind is suddenly suppressed, are facts familiar in the practice of all. The tendency in many instances to a periodical return of these hemorrhages is a point of equal importance; giving further cause for referring them either to general conditions of the blood throughout the body, or to changes of its quantity or quality in this part especially of the circulation. Here, then (admitting the exception for cases of local vascular irritation), we have a natural hemorrhage remedial in its nature; and often, by patients themselves, as well recognized in this light, as is the simple bleeding of the nose in relieving congestion or other uneasiness about the head.

These instances may be taken as illustrating others less obvious in character; such as the hemorrhages proceeding from higher parts of the intestines, either under the form of pure blood, or of those dark grumous secretions (melæna) which are to be regarded as exudations, through the lining membranes, of fluids more or less approaching to blood in their nature. These discharges, as is well known, are often to enormous extent; producing anæmia, and followed sometimes by dropsical effusions. They represent in some cases ulceration, or other grave organic disease of the viscera, and particularly of the liver and spleen. In many instances, however, they occur without any evidence of diseased structure; cease without leaving behind any injurious consequence; and even appear to relieve the system from some prior oppression or disturbance. Such is the case in certain continued fevers, where, at a particular stage, discharges of blood from the bowels form a crisis to the malady; probably by

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relieving some vascular congestion incident to the fever, if not, indeed, concerned in producing it.

Here, then, we approach to the conditions already stated regarding other forms of hemorrhage. But, even under the more serious contingency of organic disease the wisest course of practice is not always that of greatest activity. Suppression of the discharge does not remove its cause; and we possess, in truth, few means of certain avail even to control the effect. The treatment by astringents and sedatives, generally employed, is in great part theoretical; and though the employment of these means may be admissible in many cases, he must be bold who would assert their certain efficacy, or even affirm that they are never injurious under this manner of use.

There is more difficulty in applying these considerations to Hæmaturia, from the various parts and textures, and the very different causes (mechanical irritation amongst others) concerned in this form of hemorrhage. I will only remark that here also there are cases where it seems to be a simple extravasation of blood from the kidneys, without the irritation of renal calculi or other local affection;—occasionally even of periodical kind, and unattended with any obvious injury to the system. What is needful then in Hæmaturia, is a just discrimination as to the particular organs and causes concerned. If it be probable that the case is of the nature just adverted to, and the bleeding not excessive, it is better to make the treatment one of forbearance and watchfulness, than to press hastily upon the symptoms by medicines presumed to act as astringents or internal styptics; but which for the most part must be admitted as of very doubtful effect. Such caution in the outset may be maintained even when there is cause to suspect renal disease; the extravasation of blood in this way, from within the organ itself, sometimes giving an amount of relief which can in no other mode be so effectually obtained.

If these remarks upon the treatment of internal hemorrhages seem too much opposed to our accustomed notions and practice, I must repeat that their object is to inculcate a more careful discrimination as to the cases which come before us under this form. In what I believe to be a large majority of instances, we are bound to regard such hemorrhage not as a disease in itself, but as the exponent of morbid conditions either of the blood or of the vascular structure, which may in some cases be aggravated

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by the loss of blood, but in others may be, and often are, decidedly relieved by it. The nature and importance of this distinction will be at once obvious; and to none more so than those who have had large practical experience, and based their conclusions upon it.

Having laid down in the foregoing instances the principle I wish to enforce, it will not be necessary to dwell at length on those other examples where, though the morbid actions are of very different character, they may still be submitted to the same considerations in practice. Diarrhœa in its various forms,—disordered states of the urinary secretions,—excessive or faulty discharges from the mucous membranes and glands,—all involve similar questions, which the physician is bound to keep sedulously before him; the more so because natural prejudices and feelings, and the phrases current in the world, tend greatly to embarrass his judgment in cases of this kind. To maintain firmness of thought and action in the face of popular opinion —"*cogitationem à consuetudine abducere*"—is difficult, more or less, to every man; but especially to the young practitioner, who is either too timid to resist the importunities pressing upon him, or too impatient and anxious himself to exercise that forbearance, which is frequently more essential than vigor. Even in the common course of medical instruction, deviations from the natural or average state of any function are too generally inscribed with the name of disease, and their treatment taught under this view. It may be true, in the greater number of instances, that no such deviation can occur without a morbid cause being somewhere present. But this cause is often remote from the obvious and apparent symptoms, as in the many instances where morbid states of the blood are represented by the altered functions of different organs,—such alterations being not unfrequently curative in kind, even though they may create disorder and distress in their progress.

The case of diarrhœa need the less be dwelt upon, as the most ordinary experience of every medical man supplies proofs of the various causes producing it, and the very different or opposite treatment required for its relief. Take simply the two cases of excess of bile, or its deficiency from obstruction of the gall-ducts, —each testified by diarrhœa, but so different in character and cause, that no common treatment can be expedient or salutary

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for both. The mercurial which ordinarily acts as a purgative, becomes, by restoring bile to the bowels, the true remedy for the large and frequent clayey motions which indicate its deficiency. Or, take again the frequent case of diarrhœa produced by undigested or irritating matters lodged in the intestines, where the purgative which dislodges them is the sole or most speedy remedy for the complaint. Without enlarging, then, on this familiar topic, it is enough to note, in reference to our subject, that there are instances of constant occurrence in practice, where diarrhœa, if unattended with pain or other distress, may best be allowed to take its course, without the interference of remedies to restrain it; and where, in fact, the use of astringents, or other such remedies, not merely prolongs the disorder of which it is the symptom, but often produces more specific and serious mischief.

The interest of pathological inquiry into all that concerns the kidneys, has been much increased of late by what we have further learnt of the functions of this great excretory organ. One among many important lessons derived from these inquiries, respects the manner of viewing the different matters extricated from the system in the urine; and giving, by their presence in it, the extraordinary physical and chemical characters which belong to this fluid. Looking at these numerous urinary products in relation to the subject before us, they may be broadly classed into such as seldom, or never, occur in healthy urine; and those which are naturally present in healthy urine, but sometimes show themselves in morbid excess. The latter case is that to which our argument chiefly applies. The excess here denotes for the most part a like excess of the same matters, or others out of which they are formed, in the circulation; and the kidneys, in every such case, are acting a salutary part in eliminating these superfluous elements from the blood—noxious if retained in it. The practical question in each case is—are we, upon these indications in the urine, to seek correction through the system to the morbid products thus evolved there? or may we better await the effect of those natural discharges, which are themselves, through the kidneys, emptying the system of what is superfluous or noxious? This practical question fairly placed before his mind, the good sense and experience of the physician may best guide him in each particular case. I must remark here,

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that this consideration applies not only to those more acute cases, such as gout and rheumatic fever, where large deposits of morbid matters, through the urine, often form what may be termed the crisis of the disorder, but also to the chronic or habitual state of certain constitutions, where these excretions occur to such amount as might well be deemed disease, were it not that we have in their habitual discharge, the evidence of nature relieving itself. If in these instances the general well-being of the body is maintained, medicine and medical means may safely be put aside;—reserving them for those other and more needful occasions where the constitution is obviously disturbed in some of its functions, either by a suppression of these evacuations, or by an excess which the natural action of the kidneys is unable to remove.

I would again state that this reasoning applies chiefly to cases of excess in the ordinary ingredients of urine, and does not extend to those more serious cases where large albuminous or saccharine excretions through this channel testify disease in the chylopoietic organs, or a depraved state of the blood.

I will not add to the length of this chapter further than by slightly noticing another illustration of its subject, in the case of those secretions and discharges, from the mucous glands, which, under various names—bronchitis, catarrh, catarrhus senilis, &c.—do often assume a chronic form, and become habitual, more or less, to the constitution. The general argument here is the same as that which I have applied in the preceding instances. Provided there be no painful symptom present, or any obvious injury of health, we are not called upon to suppress such secretions, large though they may be, or to carry the objects of direct treatment further than to check occasional excess, to facilitate expectoration, and to obtain rest at night if this be infringed upon. To aim at more by internal remedies is often to forfeit a greater good than any that can be attained.

The physician who brings experience as well as discrimination to his practice, will, I think, recognize, and acquiesce in, the distinctions I have been seeking to maintain throughout this chapter. But they are less familiar to those young in the profession, and re[illegible]e to be dwelt upon the rather, because opposed in many [illegible]he common opinions or prejudices which are enc[illegible]ctice.

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CHAPTER XVIII.

ON SOME SUPPOSED DISEASES OF THE SPINE.

It has happened to me in practice to see a great number of these singular affections; in which certain morbid states of the nervous system, or occasionally visceral disorders, assume every character of spinal complaints, and are medically treated as such. A large proportion of these cases occur among females; and though the complaint is now better understood than formerly, yet I doubt not the instances are more frequent than is imagined. The simulation of serious disease of this nature, independently altogether of the will, is often so remarkable, that we may fairly presume many such fictitious disorders to remain undiscovered to the last.

A common foundation of error in these instances is the existence of pain, sometimes fixed, sometimes fluctuating, along the course of the spine—this pain, according to the assertions and actual feelings of the patient, very distinctly augmented by pressure; and often relieved by local bleeding, blistering, moxa, and other similar applications. There is frequently, moreover, weakness or numbness of the lower extremities, sometimes such in amount as to be construed into paralytic affection;—pain from exertion, relief from recumbency; tendency to muscular spasms;—and often difficulty in emptying the bladder. Yet all these, and other symptoms, may arise from nervous or hysterical state of the constitution without any true local affection; and in many cases they are best relieved by remedies which have no relation whatever to the spine.

It is, in truth, one of the ill effects of the misunderstanding of the complaint, that its symptoms are often greatly aggravated by the means designed for their relief; and spinal disorders even produced when not existing before, by the muscular debility due

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to long confinement, recumbent posture, and local depletions. I have seen cases, well worthy of notice, where patients having no nervousness or infirmity of mind, but suffering from topical nervous pains in some part of the spine (the result often of sympathy with internal organs), have been reduced by various treatment of this kind to a state of almost total inability of the lower extremities. The proof of the real cause is that afforded by their recovery, in very few weeks, from symptoms which have had duration for months or years;—and this recovery derived, not from recumbent posture, or from remedies applied locally to the spine; but from steel, bark, ammonia and valerian, cold salt-water bathing or washing, and exercise of the limbs sedulously persevered in and extended. In those other instances, of very frequent occurrence, where some mental infirmity is added to the physical condition producing this state, the moral remedies which the judicious practitioner may employ will greatly aid the success of the treatment—an object, which, whether it can be adequately fulfilled or not, should never be lost sight of in disorders of this nature.

I allude to these cases, not from any novelty they have to men of professional experience, but because it is important to press that due notice of them, which may exempt the young physician from the chance of error; and equally protect the patient from the mistakes of mere ignorance, and from the maltreatment of quackery and fraud.

It is unhappily true, and a fact familiar to every physician, that this class of disorders has been peculiarly the subject of such malpractices. The reason is obvious;—the result one much to be deprecated as a matter of medical dishonor. The fact, already stated, well merits attention in relation to their history, how very large a proportion of those so affected are females. As there are no causes which can adequately explain this degree of disparity between the two sexes as to actual spinal complaints; and as there are many peculiarities in the female constitution and habits of life, tending to produce the deceptive appearance of such diseases, we have here a practical intimation of the frequency of the latter occurrence, and of the importance of being well prepared for its recognition.

Both firmness and experience, indeed, are greatly required in treating these anomalous disorders. Our first steps are often

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embarrassed, not only by the fluctuating nature of the symptoms, but by the condition of the patient's mind and nervous temperament—a state of constitution generally concerned in some degree in producing the malady, and almost always aggravated in its progress. These are cases in which, if the foundation of our practice be well laid, we need in nowise be disconcerted by seeming failure in the outset. Some of the most remarkable instances I have seen of complete eventual success, have been those in which the contrarieties in the early part of treatment were most numerous and discouraging.

Ambiguous cases there no doubt are, where the topical symptoms of pain, fulness, or irritation upon some part of the spine, are sufficiently distinct to call for local remedies, on trial at least of their effects. In such instances, and equally so where reason is found for the continuance of these means, I think leeches are generally to be preferred to blisters, caustic, or other irritating applications. The symptoms, as already noticed, are most frequent in habits where there is a morbid sensibility of the whole system; and any active irritation, or even one so slight as hardly to affect other persons, will frequently be found to excite disordered actions, and disturb the whole progress of cure. I have often seen the most obvious and immediate good from removing an issue, or open blister; which had effect only in aggravating the symptoms they were intended to relieve.[1]

In cases of this nature, Opium might be used much more extensively as an external application than is usual in practice; and, according to my experience, with great benefit. That very condition of the nervous system, which renders a slight local irritation of the cutaneous nerves a source of disturbance to the whole body, gives power and value to this means as an antidote. Where true inflammation has not existed, or has been removed; and where irritation and nervous sympathies are the source of the distress thus attached to the spine and limbs, it is singular what good this application will produce:—not used, however, in the careless and inefficient way which is common with external remedies; but sedulously, and with sufficient proportion of opium in the forms employed. In another chapter, "On the Use of Opiates," some remarks will be found as to the latter point.

[1] It is a proof, however, of the strange and anomalous character of these cases, when sensorial disorder is blended with them, that external pain or irritation is sometimes even courted by the patient himself.

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These cases of supposed spinal disease are further interesting, as they belong to and illustrate an important class of diseases, which, though much more closely examined than heretofore, have scarcely yet been defined in their whole extent. I allude to the various forms of Hysteria; and especially to that frequent and curious variety of the complaint, where there is a disordered state of the sensations—or perhaps it may more truly be said, of the sensorium itself. The pains upon or along the spine, in the cases already referred to, often belong to this condition; as does also the inability to give the natural action to the limbs, which is so marked a symptom in these disorders. Sir B. Brodie (with whom I have seen many examples of them) has made the observation, that in hysterical paralysis the muscles are not incapable of obeying the acts of volition, but the function of volition is not exercised. In the truth of this observation, all that I have seen leads me fully to concur. In fact, the whole series of symptoms in these cases, whatever the remote causes, is dependent directly upon sensorial derangement; affecting irregularly certain classes of sensations, and impairing the state and action of the voluntary powers.

Many of these singular anomalies are expressly due to a depraved state of the sense of touch or feeling; testified not solely by the increased irritability already noticed, but in other instances by the sensibility being unnaturally blunted. All, indeed, that pertains to this sense (which ought scarcely perhaps to be named or described as a single one) is of the greatest interest in human physiology. The peculiarities of which I have just spoken are rendered more remarkable by what is often observed of their partial occurrence and frequent change of place;—phenomena which belong to the general theory of sensation, as well in healthy as in morbid states of the body.

In some of these cases the patient may be said to live in a sort of cycle of disordered sensations, displacing and replacing each other in the most singular way; but all removed for the most part when a real pain supervenes, the result of inflammation or other true diseased action. I have known a toothache, or a slight catarrh, or the irritation of a sprain, completely to banish, for a time, morbid impressions of the most distressing kind and long duration in the habit; all recurring when this cause of interference had subsided. The intellectual and moral faculties

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frequently partake, in greater or less degree, of the same infirmity; producing some of the most singular perversions of feeling and action which it falls to the lot of the medical man to encounter; and these sustained occasionally during a long period of time. Such cases, of which I have seen examples quite as remarkable as any I find recorded, give aid in explaining some of those anomalous phenomena which have perplexed even thinking men, and furnished in all ages large material for wonder to the credulous or uninstructed. But, as I have remarked, there are many instances where the bodily symptoms alone exist, without any such conjunction; and it is to these chiefly that I seek to direct attention in the present chapter.[1]

We are further indebted to Sir B. Brodie for the first description of that very remarkable hysterical affection of the joints, which has close kindred with the disorder now under consideration. There are, in fact, corresponding effects of a common cause, and require for the most part similar treatment for their relief. I have often found cause to suspect, as stated in a former chapter, that some of the various forms of hysteria belong more especially to the gouty temperament; ranking among its many other anomalous manifestations in the female habit.

Looking generally to the cases associated together by this common character, they may be considered as amongst the most curious and instructive in all pathology. Like insanity and intoxication, they illustrate many points in the connection of the mind and bodily organs, which are not equally obvious in the healthy state: and they instruct us, moreover, in the history of insanity itself, by displaying various partial hallucinations of mind, often traceable throughout their whole progress; and forming links, as it were, betwixt reason and madness. It is a sort of natural analysis afforded us of conditions too complex for examination, when they are fully formed and established.

[1] In the latter cases we may perhaps best seek for a cause in the reflex actions of the nervous system of the spine, which have recently been the subject of much able research by Dr. Marshall Hall in this country, and by Professors Müller and Stromeyer in Germany. To the former physician we owe the earliest distinct exposition of this class of facts, and their application to the phenomena of spasmodic disorders; a very important step in physiological science, and promising to have much future value in the treatment of disease.

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CHAPTER XIX.

ON HYPOCHONDRIASIS.

THERE is difficulty as to the place of Hypochondriasis in our nosologies, partly from the mental affection which is so marked among its symptoms, partly from the absence of any distinct local affection, by which to describe its character. This very ambiguity adds to the interest of the pathological inquiry. The malady is a real one, however difficult its definition, or obscure its relation to other bodily states. It is marked as such by the frequent periodicity of its returns, and approach to equality in the duration of the attacks; and by an identity of symptoms, designating the complaint to our medical experience, as distinctly as any other of which obvious lesions of structure do not form a part. The frequent aspect of hereditary tendency is another proof to the same effect, which cannot be passed over—especially seeing the clue it affords to certain probable relations of the disorder, which, if established, are important alike to its theory and treatment in practice.

The dyspeptic symptoms, associated more or less with this malady, have tended much to embarrass all views as to its nature and cause. Their general concurrence with the mental affection makes it impossible to neglect them as part of the complaint; while, on the other hand, the various forms and degree in which they are present, and the occurrence of analogous dyspeptic symptoms without any similar conjunction, gives cause to suppose that they are rather effects, than conditions essential to the nature of the malady. The imperfect circulation common in hypochondriasis, and the morbid direction of the mind to all bodily sensations, may well be understood as mainly concerned in producing the disordered state of the digestive organs, thus associated with it.

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As a mental affection, Hypochondriasis, like many other morbid states of mind, has its analogous condition in certain phenomena of the healthy state. It is an excess in degree, and in permanence, of those feelings which traverse at times the soundest and most equable minds; often without obvious cause, but especially, I believe, depending on incidents of digestion, sleep, or atmospheric states—disturbing and distorting reason while they last, and coloring life with dark and distempered shades—disappearing, it may be, as suddenly as they have come on, and with as little apparent reason. These moods of mind, familiar in some degree or other to the consciousness of every one, do in fact graduate into hypochondriasis in its most complete form. The essential characters are the same, and observation fills up every link in the chain between such transient conditions and deeper and more enduring disorders.

This view I have elsewhere applied to mental maladies in a more general form, believing it to afford many new and important illustrations of their origin and nature. In its relation to hypochondriasis, much striking illustration may be drawn from the fluctuations of this disorder. Even in extreme cases, it is curious to mark the sudden changes the feelings sometimes undergo, without the smallest seeming alteration in external circumstances. Not merely from day to day, or hour to hour, do such changes happen, but I have seen them almost momentary in occurrence—the mind lighting up, or darkening again into gloom, at the instant of observation. These phenomena just noted must be taken as expressing certain physical changes within the body, however difficult it may be to explain their nature or operation. That they have some relation to the graver forms of mental disease cannot be denied; similar fluctuations being more or less incidental to every derangement of mind, even such as are most permanent and severe. But one marked distinction is, that in the malady of which we are treating the aberrations and false perceptions have more frequent reference to the bodily feelings, and are, in fact, generally associated with more of bodily disorder. This distinction has some practical value in cases where the mental symptoms are severe in degree, and of long duration.

Another especial circumstance about hypochondriasis is its frequent connection with the gouty habit. This connection has

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not, I think, been enough regarded by physicians. Too prone in general to view the swelling of joints, or the immediate metastases from them, as the expressions of gout in the habit, they overlook, or only partially regard, those yet more remarkable conditions of the disorder, where the same morbid matter present in the circulation affects other parts of structure, and notably, in many cases, the brain and nervous system. The study of these varieties of disorder, due to a common cause, but modified by age, sex, individual temperament, and manner of life, is an object in every way of deep interest—a field of observation, still only partially cultivated, but capable of yielding much to those who zealously and patiently work in it.

The evidences of the connection just stated are various in kind. Referring to my own experience, I have notes of many cases where, in families strongly marked with the gouty temperament, certain individuals were exempt from the ordinary forms of the disease, but severely affected by hypochondriasis. A few cases I have known where, in gouty habits, prolonged attacks of hypochondriasis, and regular fits of gout, occurred in distinct reciprocal relation to each other—in one instance so exactly that each year, for a considerable period of middle life, either one or other disorder appeared, seeming to preclude, or make unnecessary, the occurrence of the other. The case is much more common, and equally illustrative of hypochondriacal symptoms being relieved and superseded by a fit of gout. This happens most frequently, I think, in habits where, from frequent repetition or other causes, the gouty attacks have ceased to take an acute form; and the conditions therefore coincide in part with the instances just stated of the gouty diathesis not directing itself outwardly to the limbs.

These relations, thus explicit to observation, clearly denote some community of origin; and even allowing that no certain knowledge were gained beyond the mere fact of connection, yet is this, in various ways, of much value in practice; furnishing a more secure prognosis, and suggesting various points of treatment. In truth, these connections of disease, which I have more largely dwelt upon in another chapter, are links of nature, which it is our business sedulously to seek for and apply, both in pathology and practice;—fashioning our nomenclature thereby as far as may be possible; and our classification of diseases, whenever this can reasonably be done.

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Another fact denoting hypochondriasis as a disorder of the system, or more especially of the circulating fluids, is the frequency of large and long-continued discharges of dark grumous matters; separated from the blood, either through the liver, or by the membranes and glandular structures of the intestinal canal; and especially, as I believe, by the great intestines. These morbid secretions often occur more or less throughout the malady;—in greatest abundance perhaps towards its termination. The etymology of the terms *Melancholic* and *Atrabilious*, expresses the connection, ever noticed, between a gloomy state of mind, and the presence of such vitiated fluids in the body.[1] I cannot affirm that they are always present, or in amount proportional to the other symptoms; but the concurrence is frequent and distinct enough to warrant the belief that they have essential relations to the malady. Examination of their character shows that they often contain actual constituents of the blood. They may be considered to express some altered and vitiated state of this fluid, to which state it is probable the separation by these secretions is a natural remedy. It is true, indeed, that injuries or disturbances of the brain do sometimes produce disordered secretions from these organs, not wholly dissimilar. But still there is a distinction of character in the two cases; and this distinction is further attested by the fact that the free separation of these matters in hypochondriasis, seems to give relief to the disorder, and frequently to form a true crisis to it. I have notes of several cases in which the relation of cause and effect was so marked, as scarcely to admit of other explanation.

Admitting this, as founded on experience, it becomes a strong evidence as to the general conditions of the system upon which hypochondriasis depends. We cannot yet indeed carry our interpretation further; nor, on our present knowledge, recognize in the blood those particular elements which in their nature or altered proportion, become the source of this disorder. Still less can we explain the manner of action of the blood, so changed, upon the nervous system and mental faculties and feelings—whether it be that of privation merely, or an influence

[1] The well-known couplet of Dryden expresses the same observation, current at every period,—

"The yellow gall that in your stomach floats,
Engenders all these visionary thoughts."

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positively noxious on the brain and nerves. But these difficulties belong to many analogous cases, and in nowise preclude the general view just given. Pathology in its every part abounds in examples, as explicit as striking, of the influence of the circulation on the mental functions; and even the ordinary conditions of health are subject to unceasing variation from this cause, too familiar to need illustration. The effects of altered quality of the blood are much less understood than those of quantity and impulse. Nevertheless it is certain that they enter largely into the phenomena of mental disorder; from the simplest action of blood imperfectly arterialized, to those more obscure and complex states where morbid matters, either finding access from without, or formed and retained within, disturb the functions of the brain and nervous system, and through them every organ and function of the living frame.

To this influence of changes in the state of the blood on the nervous and cerebral systems, we may perhaps refer certain particular facts respecting hypochondriasis; such as its occasional occurrence as an effect of over-nursing in women; and its frequency in certain localities—the Western Isles of Scotland, Iceland, &c.—where there is much privation and peculiarity of food.[1] The torpor of circulation, so notable in general in this disorder, may be referred either to the direct influence of vitiated blood on the heart; or to indirect effect upon this organ through the nervous system, itself so instantly affected by the state of the blood ministering to its functions. These instances do not afford more than presumption; but upon the occurrence of presumptions truth is often founded.

[1] The common occurrence of hypochondriasis among the people of the Western Islands of Scotland is a fact for which we have the authority of Dr. Macculloch. It is curiously corroborated by an observation I made in Iceland (confirmed to me by the information of the priests and other intelligent natives) of the great frequency of the disorder in that island; where similar conditions of diet and manner of life are probably concerned. The relation to scurvy is of some interest here; a disease in which there is the same depression of nervous power; and depending chiefly, as far as we can see, on non-acescent or salted diet, confinement to one spot, &c.; circumstances to which the almost entire deficiency of vegetable food, the long winters, and possibly the singular contrast between the physical condition and intellectual culture of the Icelanders, peculiarly expose this remarkable people. Accordingly, scorbutic complaints are common and severe in Iceland; treated (when even these remedies can be had) by decoctions of trefoil, juniper, the sedum acre, and scurvy-grass. In a Thesis I printed in 1811, on the Diseases of Iceland, may be found further details on this subject.

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In admitting the connection of hypochondriasis with the gouty diathesis, we go far towards the fact of its being an hereditary disorder. I have notes of many cases before me in which the attestation of this is such as to admit of little doubt. Nor is there anything here more marvellous than the transmission of other mental maladies, depending on physical causes. The great mystery of generation; and the transmission not merely of life, but of likeness, from parents to offspring, involves and includes every question on the subject. It would be futile to raise difficulties as to a part, where the whole is inaccessible to our inquiry.

Though the remark is open to many exceptions, yet I think it will be found that in persons of the hypochondriac temperament, the actual attacks of the disorder are more frequent in the spring, or early summer. Under the view I have taken of its causes, there might be some reason to expect such periodical occurrence; either from external causes or conditions originating within the body. But the fact is not explicit enough to warrant more than this notice.

The common treatment of hypochondriasis can scarcely be said to be founded on any principle, or to include any specific remedy. Bodily symptoms, especially those belonging to the secretions, are met as they occur; and certain methods, sanctioned rather by custom than success, are employed to assuage the mental part of the disorder. We must admit that what is termed *cure*, is often merely the natural periodical close of a malady, of which intermission is in most cases a character and symptom. In those instances, already alluded to, where the period of illness tends to become regular in occurrence and duration, it is singular how little this uniformity is affected by any mode of medical treatment—a fact not warranting the physician in dealing inertly or carelessly with such cases; but rather giving him motive for seeking new means to control a complaint, which is proved by this intermission not to depend on any permanent cause.

The method of treatment by purgatives is that in most general use; sanctioned in some part by the morbid character of the secretions, and in many cases producing material relief; but in other instances failing to fulfil this object, even where the circumstances are seemingly much alike. This incongruity is most

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easily explained by reference to the nature of the purgatives employed, and the manner and duration of their use. Such as are drastic and irritating often provoke more disorder than they relieve; and are ill fitted for frequent repetition in cases of this kind, where the attention of the patient is morbidly concentrated upon his bodily sensations. On the other hand, mercurial medicines, given alone, or with mild aids to their action, will often procure such altered state of secretion from the organs chiefly affected in hypochondriasis, as to afford speedy and obvious relief to the symptoms. This altered state may be of quantity alone; but under the most beneficial action, probably of quality also;—the effect consisting in the removal from the blood of those elements, on the presence or excess of which there is cause for supposing the malady to depend. But whatever the theory of action, the fact of relief from mercurial alteratives I believe to be well assured; and especially where used steadily for a considerable time, without looking for instant or strongly marked effects, and without discouragement from occasional fluctuations or relapse. It is this discouragement, ever apt to occur in the hypochondriac, which so often disturbs our practice, and renders its results uncertain or fruitless.

Colchicum is a medicine deserving more trial in this disorder than it has yet obtained. I was first led to employ it in certain cases, already mentioned, where the connection with gout was fairly marked; and with every allowance for doubtful effects, the result appeared to be favorable, in reference to the duration and severity of the attacks. The use of the remedy is readily made to concur with that of mercurials, just noticed; or with the employment of stimulants, tonics, or anodynes, where these are needed to fulfil the purposes of treatment. No such speedy effect is to be looked for as in regular gout; but with moderate doses continued for a certain time, I believe its trial to be warranted in a disorder where we possess no specific remedy.

The use of Opium in hypochondriasis is admissible, chiefly as a means of subduing the restlessness which generally disturbs the nights in this complaint, and thereby inflicts ill on each ensuing day. Its advantages, or fitness as a remedy, are doubtful beyond this; seeing that the secretions are usually much disordered, and that any undue restraint put upon them may add to the evil, by retaining in the system that which probably acts as

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a cause of disease. Nevertheless cases come before us, where the nervous agitation of the hypochondriac, closely verging on insanity of mind, does itself become a powerful morbid cause, affecting all the organs and functions of the body. And this state we are called upon to subdue—by opium, if milder sedatives are unavailing—as well for the removal of its more direct effects, as also to render more availing other means which are inert or injurious while this nervous condition exists. The questions of practice, here alluded to, must be solved by the judgment of the physician in each case; looking to the symptoms present, and further to the idosyncrasy or habits of the patient regarding opium;—a point always to be kept in mind.

Attention is not enough given to the state of the skin in the treatment of this disorder. Much may be thus done, if the methods employed be duly persevered in. But here we have to encounter the general indifference to simple and obvious remedies; and the despondent temper and feeble will of the patient, little capable of pursuing that which does not promise and procure an instant good. When these difficulties are overcome, the warm bath, or tepid shower bath, used assiduously, with frictions over the whole body, will often produce a notable improvement in the symptoms. Where the skin is harsh and dry, the use of some simple oil, or of lard, may beneficially come in aid of the frictions—a renewal of an ancient practice, which, as I have remarked elsewhere, might well form a substitute for modern remedies of more doubtful value.

Change of place and scene, so generally suggested for hypochondriasis, may reasonably be tried; though the results seldom correspond with the expectations held out, unless the change of climate be such as to alter the whole physical condition, and particularly the state of the skin and of the various secretions. Mere locomotion and diversity of external objects, do not suffice to rid the patient of his burden of troubled imaginations, except in the lighter forms of the disorder, and in conjunction with free air and bodily exercises. A more certain benefit is often obtained where we can succeed (a difficult task, it must be owned, in most cases) in altering the whole train of life; creating new impulses or necessities; and thereby urging the patient to new habits of activity. Even in this case we are still working chiefly through physical means; and giving repair and energy

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to the mental powers through those bodily functions which closely minister to their healthy exercise.

I must further remark here, that in the mental treatment of hypochondriasis a great deal of good reasoning is often expended in vain. A simple regard to the nature of the distempered fancies of the hypochondriac is proof enough that his mind is not in a state to be reached by mere argument; and experience in all severe cases of the malady teaches the same fact. The intellect seems as if closed to every suggestion from without; or if any impression be made, it is speedily obliterated by recurrence to the disordered sensations within. In such instances it becomes almost as futile to reason with the patient, as it would be in the delirium of fever or the rambling of intoxication. My experience tells me that it is generally best to express to him, in *simple but cogent manner*, the conviction that he is laboring under transient delusion of feelings;—to adhere steadily to the *same expressions*, inculcating their use upon others around him;—and to refuse all arguments, on the ground that his state of mind would render them for the time wholly unavailing. These methods, in the severe cases to which I refer, involve no real harshness; and by a little care can be so contrived as not to sanction any suspicion of approaching insanity. In slighter cases they may be modified according to the judgment of the physician; which judgment in all forms of this singular malady needs to be exercised with discretion and firmness. The disorder of hypochondriasis, as will be seen from the foregoing remarks, is in itself a little volume; recording some of the most curious relations of our physical and mental nature; and thus becoming a sort of type of various morbid conditions, by which these relations are more or less affected under the innumerable incidents of life.

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CHAPTER XX.

ON THE EXERCISE OF RESPIRATION.

MIGHT not more be done in practice towards the prevention of pulmonary disease, as well as for the improvement of general health, by expressly exercising the organs of respiration?—that is by practising according to some method those actions of the body, through which the chest is alternately in part filled and emptied of air? Though suggestions to this effect occur in some of our best works on Consumption, as well as in the writings of certain Continental physicians, they have hitherto had less than their due influence; and the principle, as such, is little recognized or brought into general application. In truth, common usage takes for the most part a directly opposite course; and, under the notion or pretext of quiet, seeks to repress all direct exercise of this important function, in those who are presumed to have any tendency to pulmonary disorders.

Yet, on sound principle, and with reasonable care, it is certain that much may be done in this way to maintain and invigorate health, even in constitutions thus disposed. Omitting some points of controversy, particularly as to what regards the mechanical influence of respiration on the circulation through the heart and lungs (points meriting, however, much attention from their importance), the free and equable expansion of the latter by full inspiration is beneficial;—first, in maintaining their healthy structure, by keeping all the air-passages duly open and pervious;—secondly, in preventing congestion in the pulmonary circulation;—thirdly, in providing more completely for the necessary chemical action on the blood, by changing at each act of respiration a sufficient proportion of the whole air contained in the lungs, and giving it more complete access to the vascular

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tissues;—all objects of great importance, and all capable of being promoted more or less by the means in question.[1]

The phrase of *exercise of respiration* may indeed be used almost in the same sense, and with the same fitness, as that of muscular exercise. Each is capable, in its respective way, of increasing the power and facility of the function concerned. It is remarkable how speedily this is often attained in the instance of the former. A person in ordinary health may so exercise the lungs by repetition of a given exertion, that what produced breathlessness at first is soon performed without effort or fatigue; a fact familiar to common observation, and attested by particular examples in those professing athletic exercises, which show not only the speed, but the great extent, to which the effect may be produced. This of course is not equally true where the liability to disease exists. But even then, the express exertion of the organs, under observance and regulation, and with due regard to the collateral circumstances of each case, may often be made a means of augmenting their capacity and power. Though the experiments as to this point are the subject of controversy, yet, as they render it nearly certain that the 40 or 45 cubic inches of a single ordinary inspiration are less than a sixth part of the air constantly present within the lungs, it is manifest that there is great scope for change in the amount of that respired, without interfering too much with the natural course of the function.

We might take lesson on this subject from some of the provisions made by nature for relief to these organs, when oppressed in their functions. The acts of sighing and yawning are notable instances to this effect. Coughing, another mode of expiration (far less to be dreaded in itself than from the inference of danger it often conveys), is an instrument in keeping the air-tubes free from obstruction; and there are many cases where the mistaken efforts of the practitioner to suppress this symptom, do actually much augment the very mischief it is sought to obviate. Other instances, however, occur, where, without any evil, coughing might be partially superseded, or lessened in severity, by the gentler exercise of respiration—so performed under the direct

[1] The ingenious suggestion of Dr. Carswell, as to the cause of more frequent tuberculous deposit in the superior lobes of the lungs, is well known, and applies to the subject of this chapter. In the volume of Sir James Clark on Consumption will be found some valuable remarks having more direct reference to it.

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influence of the will, as somewhat to exceed, without passing into labor and fatigue, the ordinary course of the function. Trial is necessary in each particular case to give assurance of the effect; and often the irritability of parts of the membrane may be such, as at once to negative any repetition. But the test as to this is so easy and certain from the observation of the patient himself, that no undue risk can be incurred in making and repeating the experiment.

With all allowance for this and other exceptions, we may still affirm the fact that good is occasionally to be gained from the regulated exercise of respiration, even when diseased actions are already going on within the lungs;—much more, where there is yet only threatening of these, from hereditary or accidental causes in the constitution. Here such exercise is beneficial, as well through its influence on the general health, as more directly by its effects on the lungs themselves, in the several ways mentioned above. There is even reason to affirm (however this opinion may contravene all common belief on the subject) that, under discreet use of the remedy, a larger proportion of good may be obtained from it in these cases, than in any other where it can be employed. It is a good we are in nowise entitled to neglect; seeing the paucity of the means we possess for dealing effectually with the most frequent and fatal form of pulmonary disease;—that which has been so often cited as a reproach upon medicine; though perhaps unjustly, when the nature and difficulties of the subject are fully taken into account.[1]

[1] In adverting to this disease of pulmonary consumption (tubercular phthisis), it must fairly be admitted that the progress of medical science has hitherto effected nothing towards a valid cure; whatever may have been done by methods for protecting the constitution against its earlier inroads, or in certain degree abating the symptoms in their progress. This, however, as remarked above, is less an opprobrium upon medicine than on first view it might appear. The more distinct knowledge that has been acquired of the tubercular diathesis—of the tendency in certain habits and in particular textures to the growth or deposit of a peculiar morbid matter—and of that more especial direction to the lungs, warranting M. Louis's assertion, that after the age of fifteen, tubercles are never found in any part without appearing also in this organ,—all this points directly to a specific constitutional disease, for which none other than a specific antidote is likely to be of complete or certain avail. Whether such will ever be found (either general for the tubercular disease in the habit, or more especially preventive of its deposits in the lungs), is still very doubtful; though the research is amply justified, both by analogy, and by the urgency of the cause requiring it. This analogy, however, does not extend to

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It is scarcely needful to say that the same regard which is paid to the quality of air in ordinary respiration, and especially under tendency to pulmonary disorder, must be extended even more carefully to cases where the quantity inspired is thus by effort augmented. Where experience has clearly shown that air below a certain temperature produces irritation in the air-passages, it is certain that this cannot be fitted for the exercise of respiration as here recommended. And the same with respect to a humid atmosphere; under which any given degree of cold produces much more effect on an irritable membrane, from the medium through which it is applied. These points, indeed, though adverted to as maxims in the treatment of phthisis and other pulmonary diseases, are not sufficiently made the subjects of trial in particular cases. Two or three experiments, readily devised for each case and unattended with risk, might suffice to decide a question which applies alike to the preventive and curative practice in these disorders, and throughout every part of their progress.

As respects the modes of exercising the respiration, they should be various, to suit the varying powers and exigencies of the patient. The "*clara lectio*" is one of very ancient recommendation, the good effects of which are not limited to this object alone. It might be well, indeed, were the practice of distinct recitation (such as implies a certain effort of the organs beyond that of ordinary speech) more generally used in early life; and continued as a habit and exercise, by those especially whose chests are weak, and who cannot sustain stronger exertions. Even singing may for the same reasons be allowed in many of these cases; but within much narrower limits, and under more cautious

the nature of the morbid matter itself, or its relation to other specific remedies. Nothing has yet been ascertained as to these points, which can be considered even a plausible clue; and the discovery, if made, is more likely perhaps to be the effect of some happy accident, than of any concerted plan of inquiry.

These difficulties, which can be rightly understood by medical men only, become a vindication for that which the science has not yet attained, though sedulously sought for. Meanwhile, and in conjunction with these efforts, attention has justly been directed towards the means best calculated to guard constitutions so disposed against the active ingress of the disease. This is obviously the only alternative as a principle of treatment; and it has been successfully pursued in many ways, so as undoubtedly to place the preventive practice in phthisis on a better footing than at a[illegible]rmer period. More, however, is doubtless still attainable; and the subject [illegible]sent chapter is one which furnishes evidence to this effect.

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notice of the effects. If such caution be duly used as to posture, articulation, and the avoidance of all excess, these exercises of the voice may be rendered as salutary to the organs of respiration, as they are agreeable in their influence on the ordinary voice. The common course of education is much at fault in this respect. If some small part of the time given to crowding facts upon the mind, not yet prepared to receive or retain them, were employed in fashioning and improving the organs of speech, —under good tuition and with suitable subjects of recitations,— both body and mind would often gain materially by the substitution.[1]

One or two remarkable cases are known to me, where a constitutional tendency to asthma, showing itself early in life, has been subdued to a great extent, and without ambiguity as to the effect, by thus tasking the chest in certain regulated efforts, of which recitation formed a part. However obscure the causes of this disorder, I doubt not that more might be done in prevention of its attacks by methods thus applied, and sedulously continued. The great difficulty in making such means of avail, is that of obtaining due persistence in their use.

Of actual muscular exercises directed to this function, that of full and repeated inspiration in the free air is perhaps one of the best. The exercises which open the chest, as it is termed, are salutary, provided they are not such in amount as to hurry the circulation, oppress the breathing, or occasion too large and sudden expenditure of muscular power. These precautions do not always receive the notice they deserve in relation to exercises of the chest; and especially in the case of those of feeble habit and disposed to pulmonary disease. If the exertion be such in kind or degree as to quicken materially the action of the heart, and hurry the respiration, no good is obtained, and much evil

[1] Those who are familiar with the strong sense and sound maxims of Quintilian, will on this point, recur with pleasure to his instructions regarding the culture of the voice, pronunciation, and manner of speaking, in the latter chapters of his first book. This is not the sole subject on which the physician may find valuable suggestions in his writings.

In some of the French military schools, the young officers are now taught to give the word of command in the open air with such strong and clear articulation as is fitted to their future function in life. I learn on good authority, that this practice is found very beneficial to the freedom and power of all the functions of the lungs, as well as of the voice in particular.

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may be incurred. These functions are the best, as well as most ready, tests we can apply, to regulate what may expediently be done. Altered they are in degree even on the slightest exertion; but they ought not to be so to such extent as to prove distressing to the sensations of the patient at the time, or to leave the feeling of languor behind.[1]

The breathing, therefore, may best be exercised in these cases by full inspiration when the body is otherwise at rest, or in slight and easy movement. The lungs will thus be more completely filled than by hurried respiration, and without any evil to countervail the good. It is from this cause, chiefly, that exercises on horseback are for the most part more beneficial to persons having weak chests than those on foot; in which greater muscular exertion is required, and where the diaphragm particularly is brought into more hurried action. But there are cases where neither mode of exercise is practicable; yet where the gentle exercise of inspiration by voluntary effort, so as to bring more air into the lungs than is done by the common act of breathing, is advantageous even to those who are deeply under the influence of pulmonary disease.[2]

It is worthy of remark that rapid motion through the air, on horseback or in an open carriage, is often felt as more beneficial to those in ordinary health than a slower rate of movement. It may be that the air thereby gets more thoroughly into all the bronchial cells, removes what has been stagnant there, and produces more rapid and complete arterialization of the blood. This is obviously the same case as that of moving against a wind; which is generally found to be invigorating in its effects, where the wind is not so strong as to produce much muscular

[1] This is particularly to be guarded against in those gymnastic exercises, which have had their periods of fashion as well as other less commendable remedies; and have often been carried to injurious excess, both in this point, and in the over-strain given to the several parts concerned in motion. Rightly applied and limited, they have great value in various ways for the maintenance of health, and the restoration of parts enfeebled or altered by disease.

[2] A caution regarding exercise in such habits may be given here, which I do not recollect to have seen mentioned, though founded on a remarkable fact. This is, the increased difficulty of breathing for a time, on stopping suddenly from any exercise by which the respiration has been much hurried. Without entering into the cause, it is enough to mention this circumstance as suggesting the precaution of making the change more gradual, when a state of breathing occurs which in [illegible] cases ought especially to be avoided.

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toil in facing it. Other animals evidently share with man in the bodily feeling thus produced.[1] There is a distinct difference of sensation in the opposite case of moving together with the wind, and at the same, or quicker rate than its motion; and I have even known instances where a certain degree of faintness appeared to be thus produced, obviated speedily by changing the direction of progress.

It is not improbable that these effects depend in great part on the different amount of change effected by the air upon the blood. Attention has scarcely enough been given to the rapidity with which such changes often take place; though both experiment and observation show how sudden, as well as singular, is their influence upon all parts of the frame. Everything which tends to give full ingress of air into the lungs contributes to this effect; which may best be described by saying that all the functions are brought into greater activity, and the power of vitality augmented throughout the whole being.

The remark just made as to the effect of rate of movement through the air without muscular exertion, might be usefully applied to many cases in practice, beyond those in which the chest solely is concerned. In dyspepsia, for instance, where the circulation is for the most part feeble, and both body and mind inapt to exercise, much good is attainable in this way; and those fitting changes in the blood, which are interfered with by the whole manner of life of the dyspeptic, may be procured without efforts to which it is difficult habitually or sufficiently to rouse him. The frequent conjunction of dyspepsia with the earlier stage of pulmonary disease, affords suggestions here which merit much attention in practice. Adequate change of air by respiration, though it may not reach cure, yet as far as it goes is remedial for both.

In cases of this kind, and still more in Chlorosis, it is worthy of note how very inertly the acts of respiration are often performed, and how little alteration is made in the air within the lungs by each repetition of the act. The chest is hardly seen to rise during common inspiration; the patient seems scarcely capa-

[1] A coachman will say that his horses do not breathe so well, that they sooner become faint, with the wind blowing behind them, though the positive draught be less. Something is due here to the heat from exercise not being so freely carried away; but this will not explain the whole of the effect observed.

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ble of aiding it by voluntary effort; and short spasmodic breathing comes on upon the slightest bodily exertion. I have seen many cases of this kind, where I could affirm, upon simple outward inspection, that not one-half the number of cubic inches of air, which belong to natural inspiration, entered into the lungs. Here it is manifest that the proper changes in the blood, by oxygenation and the removal of carbonic acid, do not take place, and that all the organs and functions of life suffer in effect of this privation. Many of the symptoms in the singular class of disorders to which we refer—even some of those affecting the mind—are certainly derived from this source, whatever be the original causes in the state of the uterus, of the brain, or other parts of the nervous system. And it must be kept in view, that the deficient change in the blood (testified in chlorosis by a want of the right proportion of fibrine and red particles, and by lessened power of coagulation) has effect not only upon the secretions to which this fluid directly subserves, but also, as we have every reason to suppose, upon the vigor and equality of the circulation itself. The action of the heart, the state of the capillaries, and the altered production of animal heat, may be received as the more obvious proofs of a fact than which there are few of greater importance in the animal economy.[1]

I doubt, indeed, whether this point of the due arterialization of the blood, familiar as a physiological doctrine, has been enough considered by physicians in relation to the prevention or treatment of disease. Venous blood circulating in the arteries is a cause of speedy death to the functions of the brain; and thence indirectly (to say nothing of its simultaneous effect on other organs) to the whole body. Between this state of venous blood and that perfectly arterial there may be every possible grade of change. Such intermediate variations we

[1] Some interesting experiments by Dr. Alison (*Reports of the Fifth Meeting of the British Association*, 1835) lead to the presumption that the oxygen of the air has effect in exciting the flow of blood through the capillaries, independently of any action on the vessels themselves; confirming in this the curious notices of Haller as to the *drawing* of blood towards any part where an opening is made in a vessel, so as to admit the contact of air with the fluid within.

Though we have no proof that the physical peculiarities of chlorotic blood are owing to deficient respiration alone, yet there is reason to suppose this cause materially concerned; more so, perhaps, than is usually adverted to in histories of the disorder.

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know to exist in many cases, and most distinctly in some forms of disease. It is certain that each proportion between the two extremes must have its definite effect, for good or ill, upon every part of the system—partly by altering the power and rate of the circulation itself—partly by ministering a fluid of different chemical composition for the nutriment, secretions, and other functions of the several organs—principally, perhaps, by the more direct influence on the nervous system just referred to. The great agent in determining these changes in the blood is undoubtedly the respiration. And commanding this function as we thus do to a great extent, both in the quantity and quality of the air inspired, we are bound to take more advantage than has yet been done of all which this important power can afford us for the prevention or relief of disease.

The disappointment of earlier and more sanguine expectation as to the medicinal value of the protoxide of nitrogen (*nitrous oxide gas*) has had the effect of withdrawing attention too much from this remarkable agent. That which can work such extraordinary effects upon the nervous system, affecting even the mind with a new sort of inebriety of thought and feeling, must, on the most assured grounds of analogy, be deemed capable of some remedial action, whatever its nature or degree. The inequality of influence upon different persons, and the disagreeable influence upon some, are circumstances common to all powerful remedies we possess, and not fairly to be admitted in argument against its eventual use. Looking at the function of respiration under any view, it can scarcely be doubted that cases must occur where it may be important to add to the proportion of oxygen inspired; and the evidence of effects from this particular compound is sufficient to suggest it, under modification, as the most expedient method of attaining the object.

Whether oxygen under its allotropic condition of Ozone may even become admissible and useful as a medicinal agent, is a question still untried to our experience. It is scarcely conceivable that a modification thus remarkable, of an element so important as oxygen to every function of animal life, should not have some distinct influence, even when partially blended with atmospheric air by the natural changes around us—still more influence, it may be supposed, were the proportion of Ozone artificially increased, as might readily be done by the means we possess. This is one of the many things open to future inquiry.

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CHAPTER XXI.

ON SOME POINTS IN THE PATHOLOGY OF THE COLON.

I DOUBT whether all the functions and disorders of this bowel have obtained proper attention in practice, or their influence in producing disorder of other parts of the body been sufficiently regarded. The Colon is often viewed merely as a part of the alimentary canal, with the office of simple transference through it, after the more important stages of digestion have been completed. It is certain, however, that there is much beyond this, in its actions both of healthy and morbid kind. Its peculiar situation, connections, and flexures—its great extent of internal surface, multiplied by the bands, folds, and other inequalities of the lining membrane—its liability to unequal distension, contraction, or stricture—and the variety of secretions from the glands and vessels of its inner surface—all concur in giving great importance to this intestine in the animal economy. From its continuity with the rectum, many circumstances are common to the two portions of the canal, and to these the following remarks equally apply. But each part has its peculiarities, and those of the Colon, for obvious reasons, have hitherto engaged less than their relative share of notice.

The nature and amount of change in the alimentary matters, as they pass through this part of the canal, are yet not wholly known; nor are we assured as to the opinion lately propounded, on the authority of Dupuytren and other accurate observers, that the large intestines are peculiarly concerned in the digestion of the vegetable part of food. That there is power of absorption into the system through some part of their structure seems certain; and, equally so, that this power may be much augmented by especial circumstances. But as a general fact it is probable that this portion of the intestine is much more an

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organ of secretion than of absorption; and further, that these secretions are not merely subservient to changes in the matters passing through, but useful or necessary in removing other excrementitious or hurtful parts from the system. It cannot be doubted that much of what is healthily voided by the bowels is, in fact, a secretion from the follicles of the lower intestines, and not simply a residuum of food. Under this view, the Colon becomes one of the excretory organs in a more direct sense; and, admitting such double natural function, the study of its morbid conditions is one of much interest to the medical practitioner.

I shall not here speak of the disorders or diseased states of the Colon, more familiarly known;—spasms; acute or chronic inflammation of its mucous membrane; dysentery; the various forms of ulcerated or gangrenous intestine; induration or thickening of its coats, &c.; but confine myself chiefly to a few points less recognized in general practice.[1]

[1] It is difficult, however, to mention ulceration of the Colon (connected as so often happens, with similar state of the adjoining membrane of the ileum), without adverting to the singular and sudden influence which this state of bowel seems occasionally to have on the system at large—the condition of typhoid fever and sensorial disorder speedily brought on, and often with fatal issue. I have known this termination so rapid in some cases, even within a few days of the first seizure, and with so little severity of local symptoms, that it was difficult to consider the ulcerated state of the intestine as other than a partial effect of the constitutional disorder. This view, however, is not equally warranted by other cases; and we have still much to learn on every part of the subject. The value of research directed to it, is well indicated by the statement of Andral, that, out of ten cases of acute malady occurring in other parts of the body, there is an average of eight, in which derangement is found either in the texture or functions of the alimentary canal. This statement may be too precise for the many ambiguities depending on our ignorance of the primary seats of disease, but it is nevertheless true to a remarkable extent.

The whole subject of gastro-intestinal disease has additional interest in its connection with the modern doctrine of some pathologists (of whom Broussais is the most conspicuous), who regard all the varieties of continued fever as merely symptomatic of certain morbid states of inflammation of these membranes, or their follicles and glands. This opinion, were it true, would go far to decide that still unsettled question which lies at the root of all pathology, whether there be any true idiopathic fever wholly separate from local irritation or lesion of texture? But, besides that the arguments for this doctrine are manifestly too exclusive in their direction to the intestinal canal, they do not furnish any adequate proof that the affections of its membranes are really prior in date to the febrile symptoms, even in some of the fevers to which the term of gastric seems especially applicable. The uncertainty

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Many morbid states attributed, with the too frequent vagueness of medical language, to the stomach and liver, are chiefly, as I believe, connected with the Colon as their seat and source. This remark applies alike to disorders of secretion, and to those in which the symptoms depend either on nervous sympathy, or simply on the mechanical effects of juxtaposition and attachment of parts. As respects the first of these cases, it cannot be doubted that many of the various egesta from the bowels, usually termed bilious, and treated as such, have no other relation to bile than that of mere admixture. They are separated from the vessels or glands of the larger intestine by exudation or secretion;—act upon the contents of the bowel, as well as upon the living parts of the system, according as they deviate in quantity or quality from the healthy state;—and, in the excess of such deviation, indicate a state of the glands and secreting membranes, or it may be of the system at large, connected with some of the most important diseases of which we have knowledge.

The peculiar matter, resembling coffee-grounds, which sometimes comes away in such large quantity from the bowels, is often described as disordered bile, though I believe it to be separated in great part in the lower intestines. Those secretions also which resemble chopped grass or spinach have probably the same origin; and even of the liquid which is called green bile it is doubtful what proportion may come from the liver. The color of what passes from the bowels is often the effect of changes taking place within the intestine itself. This may readily be conceived, looking to the many materials present for chemical combination, both in the egesta and secreted fluids; and to the facility with which color is changed from very slight causes, such as a small excess of acid, in matters thus composed. We know from experiment that such changes are readily effected by artifi-

arising from this source is best denoted by the fact, that almost every argument employed to prove the derivation of fevers (and especially those of typhoid type) from disorders of the intestinal membranes, may be equally directed to show their immediate origin in affections of the brain. The parity of weight, in reasoning thus differently applied, gives strong presumption that neither opinion can be really just. And adverting to all the circumstances in the history of fevers—their varieties, origin, course, and termination—it is impossible not to find in the changes of the blood itself a source equally probable as those just mentioned, though perhaps as little to be determined by any precise evidence. No author has better treated this subject than Dr. Alison, in his Outlines of Physiology and Pathology.

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cial means, as well in the urine, as in the matters passing from the bowels;—and, though not having certain knowledge of the same chemical agencies going on in the body, many that are analogous may fairly be presumed to exist.[1]

The liver in fact, important though its functions and diseases are, is often charged with more than belongs to it; and the provocation of mercurials applied and continued under this view, in cases where the coats of the bowels are chiefly, or exclusively, the seat of the action which it is sought to remedy. In some instances benefit may arise from the practice, though founded in error as to the direction of treatment. But if the appearance of the matters separated from the bowels be made, as it often is, the exclusive argument for persistence in these medicines, they may be carried to an injurious extent, and interfere with the natural course of actions really salutary to the system.

In like manner, those other secretions which have the character of minute specks of blood disseminated through a fluid, or sometimes of a black matter almost as deep as ink in color, may be considered chiefly owing to exudation from the mucous membrane of the bowels. These secretions or exudations, which are found by the microscope to contain elements of the blood, may be extravasated, it would seem, from the membrane of any part of the intestines. But I have seen instances where products of this description, coming away in large quantity before death, have been found on examination everywhere lining the coats of the Colon, without any appearance of similar kind in the smaller bowels.

Secretions, of the general character just described, do in fact show themselves in every grade of change from pure blood to appearances widely different; with certain evidence that they are separated from the intestine itself, whatever name be given to the process by which this takes place.[2] It is important to distinguish these from mere bile, and to note their various appearances; partly as concerns questions of treatment directed

[1] The researches of Tiedemann and Gmelin in Germany, and those of Dr. Prout in our own country, afford many valuable results on this subject.

[2] Andral describes the separation of this black matter (*mélanose*) from the coats of the intestines by the phrase "la membrane muqueuse la laisse suinter."—*Anatomie Pathologique*, vol. ii. No better description than this can probably be given.

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to the membranes; equally so, because these secretions are often the index to changes taking place in the system. In some cases they precede and indicate a fatal event; depending here, it may be, on a particular state of the exhalants, akin to that which produces cold sweats upon the skin; or possibly in still greater part on changes in the state of the blood itself. In other instances it would seem that these large discharges of dark grumous fluid (of different quality doubtless from the preceding) actually relieve the blood from some morbid matter, loading and disordering the whole system, and particularly perhaps the chylopoietic organs and venous circulation. This subject, in fact, must be viewed in especial connection with the distribution, functions, and disorders of the *portal system;* a due regard to which will aid us much in interpreting many obscure points in pathology. The apparent relation of certain of the matters, so removed, to the morbid products which have been classed under the name of melanosis, is one of the results derived from recent examination into diseases of the blood.

The alteration in general health, which often follows these discharges, undoubtedly justifies our placing them among the various actions (whether assuming the aspect of disease or not) by which important changes are made in the mass of the circulating fluids, and through these on the various functions of the body. I have seen many cases of hypochondriasis of long duration, many also of protracted disturbance in the action of the heart, speedily and wholly relieved in this manner. We have other authority for this view in the peculiar quality of some of these excretions; rendering them, if long retained, exceedingly noxious to the body in which they are generated; producing sometimes great irritation, in other cases extreme depression of the vital powers. And there is further evidence here in the analogy to those curious fæcal excretions from the roots of various plants, poisonous more or less to the vegetable life by which they are produced.[1]

[1] The observations of Macaire on these remarkable excretions from the roots of plants are valuable to agriculture, as well as interesting in the analogies they afford to animal life. On the latter point I have notes of several instances, showing the noxiously sedative influence of some of these matters generated within the body, and thus separated from it. One case is now before me, where, in a vigorous young man of two-and-twenty, the pulse, habitually about seventy, was brought

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The treatment of the case mentioned above is not without many difficulties, and we have reason to believe that mischief is often done by a mistaken practice. To deal with these instances as with common diarrhœa, by opiates and astringents, is an injurious interference with actions beneficial or needful in their result. On the other hand, irritation, by medicine or otherwise, to the vascular surface which is the source of these peculiar excretions, may inflict mischief of another kind, and equally prevent a salutary issue. The rule of practice here can only be furnished by the observation and experience of the practitioner.

These difficulties, in truth, are common to cases of diarrhœa of more ordinary kind, arising out of the complication of the causes concerned;—the various ingesta received into the stomach—the different secretions poured into the alimentary canal, as well from the large glandular apparatus of the liver and pancreas, as from the innumerable mucous follicles in every part of the canal—and further, the great extent of vascular surface from which fluids may be separated by exudation or otherwise. In effect of this, a great variety of division and nomenclature has arisen as to these disorders; adding much to the embarrassment of the student, and increasing the chances of error in practice. It will probably be admitted by all who give honest expression to their experience, that even in these very familiar complaints the rule of treatment is less certain and consistent than in most other disorders; fluctuating perpetually between the doubt whether the diarrhœa present is to be checked by direct means, or allowed to proceed on its course;—whether medicines should be given with intent gradually to alter the secretions; or, with a bolder hand, to remove at once such as may be disordered or hurtful. This uncertainty of practice, of which I have spoken in a former chapter, can only be obviated by experience, and by a careful observation of what is mere mechanical irritation—what, vitiated secretion from other topical causes—what, the separation of noxious matters from the blood through the liver or intestinal membranes. Technical rules here may be too minute, as well as too general and vague,

down below forty, and rendered very irregular, by the passage through the intestine of a large quantity of that peculiar secretion resembling black oil; the pulse rising again immediately after it was removed from the body.

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to be useful in practice. At all events, they can never supersede individual discrimination.[1]

Some of these remarks, just made, relate to disorders of the small intestines as well as to those of the Colon; but the cases are so numerous in which this bowel is especially affected, and to which these questions of treatment directly apply, that it is impossible not to notice them as an essential part of the subject before us.

The disturbances produced in the body by what may be called the *mechanical conditions* of the Colon, deserve some notice; particularly as they are sometimes mistaken for more serious disorders of other organs. The whole subject, indeed, of sympathetic and reflex actions, produced by unequal distension, or change of place, of portions of the intestinal canal, is curious and instructive in numerous points of pathology. Their influence upon the nervous system, even as connected with the common process of digestion, is familiar to us in every moment of life. It is more strikingly shown in different states of disease, especially in the female habit; as well as during the period of infancy, when the nerves are more sensitive to all automatic impressions, and less under the control of those functions of the brain which afterwards govern so large a part of the muscular system.

As respects the Colon, these effects manifestly depend in part on the contiguity or attachments of this intestine to other organs; making the latter liable to be affected by the distension

[1] In all general reasoning on the disorders of the abdominal viscera, it is needful to estimate fairly what is still wanting to an adequate knowledge of the healthy functions of these organs. The stomach is now perhaps best known; but chiefly through researches of recent date. The progress in the minute anatomy of the liver, for which we are chiefly indebted to Mr. Kiernan; and the many valuable experiments, and bold speculations, applied to its physiology, have left us yet in doubt as to the specific uses of this great organ in the animal economy. The functions of the spleen are still matter of vague conjecture. Numerous questions remain to be solved as to the especial functions of the several parts of the alimentary canal. And though much more has been done towards the history of the kidneys both in sound and diseased state, yet the relations of the urine, in its quantity and properties, to the various changes occurring in other parts of the body, still offer singular difficulties to the physiologist.

I have elsewhere referred to these and other deficiencies, for the illustration they afford of the difficulties attending all medical evidence.

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which often occurs in the whole, or parts, of this bowel from temporary causes. Its peculiar course through the body renders some of these connections very important in pathology; since not only the stomach and other parts of the alimentary canal, but also the action of the heart and the respiration, are liable to great disturbance from this cause. The latter effects are more especially produced when the transverse arch, as so often happens, is distended throughout its length; forming a tight girth across the body, and pressing directly or indirectly upon the diaphragm, stomach, duodenum, and even the large vessels underneath. Though there is often ambiguity from the distension of the stomach itself, yet in many cases it is obvious that the Colon is solely concerned; either through air confined in the bowel, or from more solid matters compacted in some part of it; thus becoming one of the various circumstances in digestion by which the heart's action is disturbed, frequently so much as to alarm the most experienced practitioner.

Looking to the irritation of the stomach thus produced by distension of the Colon, we shall find many states of disorder, supposed to belong to the former organ, to be really due to this cause. The distress so common to dyspeptic patients in the course of the night, and obviously connected with the state of the bowels, seems to belong to this part of them more especially; the recumbent posture, for obvious reasons, increasing all disturbances which depend on its distension, and pressure upon adjoining parts.

The close attachment of this portion of the Colon to the stomach and duodenum, affords reasonable explanation of many other symptoms of disordered digestion; and of the morbid sensations or actions rapidly diffused through distant parts of the alimentary canal. A distended state of the Colon may disturb an earlier stage of digestion; either mechanically by pressure on parts nearer the stomach; or indirectly through the system of nerves, by which these various organs are associated in one common function. Both these causes must be kept in view in treating the disorders to which they are severally subject. The actual complexity of such complaints may well embarrass the young practitioner; seeking to unravel, by technical names and descriptions, symptoms which no nomenclature can reach.

Disordered states of the Colon are not unfrequently mistaken

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for complaints of the kidneys; and there is the greater liability to this mistake from the influence they really have (either mechanically, or by some cause of sympathetic irritation, or by changes in the circulation), in disturbing the state and secretion of the urinary organs. Whencesoever it arise, so close and frequent is this connection, that we may always expediently begin the treatment of apparent disorder of the kidneys by full evacuation of the larger intestines; secure that we shall obtain alleviation in this way, if not entire relief. Few of the means, especially directed to the urinary organs, are so effectual as those which operate upon them through this part of the intestinal canal.

Many pains in the back and loins, which pass vaguely under the names of lumbago and rheumatism, are distinctly to be referred to the same cause. The effect of treatment here is usually the most certain proof; purgatives and injections relieving these symptoms speedily and effectually in many cases, while failing in others. The very peculiar pains in the same parts, which attend the whole course of acute dysentery (and which are by no means sufficiently indicated in the common descriptions of this disorder), give further evidence how remarkably the morbid state of these bowels affects the adjoining textures.[1]

Cramps, and other spasmodic and painful affections of the lower limbs, are a frequent effect of the mechanical distension of different portions of this bowel; perhaps also, of disordered and acrid secretions lodged within, or passing through it. Of the latter we obtain proof in the very common concurrence of these symptoms with dysentery or ordinary diarrhœa. The acid also,—which, according to recent observation, is for the most part predominant in the larger bowels,—may be in such excess as to produce various disturbance by sympathetic irritation. The Cœcum, for obvious reasons, is the part most liable to distension: often from solid matters accumulated there in extraordinary quantity. Its effects when thus loaded, even upon distant organs, are so various and considerable as to require discrimination in practice. I have seen more than one case, where pains

[1] Those physicians who consider that dysentery expresses not so much a state of the intestinal membranes, as a more general disorder of the chylopoietic viscera and of the portal and mesenteric circulation, may find somewhat different explanation of these pains. It is probable, indeed, that they are derived from different sources.

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were produced in the right leg, severe and constant enough to suggest the idea of more permanent disease in the joint or limb.

There is some difficulty in understanding the cause of those circumscribed swellings, to which all parts of the intestines seem liable; and which are sometimes so contracted in extent as to convey to examination the idea of hard, well-defined tumors, occasionally deceiving for a time by the resemblance. These swellings, as well as all other distensions and inequalities of the bowels, are more frequent in the female habit, and seem in certain cases connected with the peculiarities of the hysterical temperament. As the passage along the intestine, though impeded, is not generally closed in these cases, we must suppose that its coats are distended into a sac out of the direct course of the canal;—originally, perhaps, from air generated suddenly or detained in this part of the intestine;—or from the irritation of other more solid matters producing partial contractions, with relaxation of the intervening membrane and loss of power in the muscular coat. The sudden and unequal distension of the bowels from certain articles of food, or during the action of irritating medicines, illustrates the nature of these more circumscribed swellings. They are often relieved by a single active purgative (with which creasote or cajeput oil may beneficially be combined): but the habit of using frequent laxatives, by weakening the canal and rendering its motions irregular, increases the tendency to such disorders.

The whole subject of Tympanitis, to which these circumscribed tumors have relation, is under some obscurity. It is certain that there occurs occasionally a powerless or palsied state of the intestine, over more or less of its extent, concurrently with which tympanitic swellings come on;—whether as cause, or effect, is not well ascertained. I think it probable that each supposition may be correct in part. The distension of any portion of the canal beyond a certain degree clearly impairs or destroys its power of action; and still further distension may thereby be produced. But we have reason to suppose that this state of intestine (especially when general throughout the canal), arises from other causes; such as belong either to the nervous system of these organs, or to the effects of inflammation on the surface of the lining membranes. It is not easy to discriminate among

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these cases of mutual action; nor is it often attempted, though some points of practice might be founded on the distinction.[1]

The various distressing sensations and disturbance to other parts, which thus arise from undue distensions of the canal of the Colon, almost defy enumeration. They form part of the sufferings of the dyspeptic, affecting all parts of the nervous system; and are unhappily aggravated in general by the means which such patients are prone to employ. I have elsewhere spoken of the importance to the healthy state of this bowel and of the rectum, that there should be a proper and equable distension by the contents passing through them; and of the evils arising from habitual or frequent purging, in reference to this point, as well as to the state of the secretions from the coats and glands of the canal. I again allude to the subject from persuasion of its importance in practice. I may add further that the habit in question seems occasionally to be chargeable with that opposite state of the Colon, equally or even more distressing in its results,—viz., the permanent contraction of the canal for more or less of its extent, particularly along the transverse arch of this intestine.[2]

I have often sought to ascertain what are the appearances of the tongue, especially indicating disordered states of the Colon. Such have been described, though vaguely; but it is certain that they exist, seeing how variously and delicately this organ gives testimony to changes in the long tract of internal membrane, of which it is one of the terminations. On its general value as an index in disease, some remarks will be found in the subjoined note.[3] So many, however, are the complications in the present

[1] Dr. Abercrombie, in his work on the Diseases of the Abdominal Viscera, relates one or two cases in which large tympanitic swellings were speedily removed by the use of electricity.

[2] In two of the most remarkable instances I have seen of this organic disease, there had been a long habit of using large and very frequent doses of salts;—in one of these cases to a greater extent than I have ever before known it. This may have been casualty, but it is worth noticing.

[3] Among the external indices of change within the body, the tongue is perhaps the most valuable. Constantly as it is referred to in practice, we are still only partially informed of all the diagnostic marks it affords. Scarcely can the pulse compare with it in the extent, variety, and accuracy of these indications; which are not limited to disorders of the membranes and secretions of the alimentary canal, or to the presence of fever in its several forms; but extend also to the nervous power, of various states of which it affords very correct evidence. The sensibility, the volun-

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case, chiefly from the state of the intervening parts, that I have never been able to satisfy myself with having obtained any well-defined knowledge from this source alone.

tary powers, and even the more intellectual functions are often exactly thus interpreted. (*Αἱ τρομωδεες γλωσσαι σημειον ουκ ἱδρυμενης γνωμης.—Hippocrates.*) No one can doubt this who is familiar with the appearance of the tongue under great debility; slowly and with difficulty put out of the mouth; withdrawn with equal effort; tremulous the whole time; its surface parched with stoppage of all secretions; its sensations blunted or depraved.—There are various parts of morbid anatomy which less merit the care bestowed on their representation or description than do the disordered states of this organ. The fugitive nature of the appearances, and their less fixed relation to disease from the variety of parts they represent, are obstacles to the easy interpretation of them. But the opinion may be repeated, that the physician cannot better study any set of signs than those afforded by the tongue, the palate, and fauces; the terminating portions of that inner surface along which so many actions are carried on, both of health and disease. The Observations of Dr. Beaumont on the precise and uniform relation between the tongue and stomach, in the curious case of St. Martin, might well justify this remark, were it needful to seek other authority than the daily experience of every medical practitioner.

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CHAPTER XXII.

ON THE ABUSE OF PURGATIVE MEDICINES.

In several former chapters I have had occasion to allude incidentally to this subject. But such allusions are insufficient in commenting on a part of practice, where the use of a most important class of remedies is often converted into an abuse, by wrong and indiscriminate employment;—without due regard to the natural powers of the body in health, or to the peculiar course of morbid actions in disease. If asked whether the use of purgative medicines, beneficial beyond all others under certain conditions, is not carried too far in modern English practice, I must affirm my belief that it is so; and each successive year of experience strengthens this conviction. It may be noticed as one of those instances of fashion in medicine, so largely sanctioned by reason and experience as to last beyond the ordinary term of mere novelties; yet so far carried into excess, that the same reason and experience require much abatement of the abuse. Two or three works of merited reputation on this subject;—the success of antiphlogistic treatment in many diseases which had formerly been dealt with otherwise;—and especially the great benefit derived from purgatives, in certain cases where such treatment had before been deemed injurious,—all these circumstances concurred, about the same time, to give new reputation to this mode of practice. It may be added, that the simplicity of the treatment favored its adoption by medical practitioners; while it had further countenance from the prejudices of the world, always prone to accredit a method from which there are obvious and speedy results, however doubtful the benefit thence derived.

We cannot wonder that fraudulent advantage should be taken by empirics of a feeling thus general and so far sanctioned by

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the habits of more regular practice. The mischiefs which have thence arisen are, in fact, notoriously great. One form of purgative drugs succeeds another in noxious fashion; a fictitious need is created; and the functions of nature are injuriously supplanted even under reputed health, by the compounds of quackery and fraud. This evil can only be lessened by a more reasonable employment of these medicines among the profession at large; and hence a further motive for weighing well what is really their use, and what their abuse, in our present practice.

One of the greatest abuses undoubtedly is, the system of giving daily purgatives, and insisting upon daily evacuation; making this the habitual management in health, and the invariable treatment in disease. Under both conditions, it is a notion fertile in mischief. Looking first to that of health;—it is certain that the natural constitution of different persons varies greatly as to the action of the bowels, and not less so the constitution of the same persons at different periods of life. To seek, therefore, by medical means for anything like a common rule, is in most cases an absurd and injurious interference with the natural functions. The instances are frequent of individuals in perfect health who have action of the bowels only every second or third day. I have known cases much more remarkable from the length of interval; yet without the impairment of a single function of life; and where mischief even was produced by any frequent or forced interference with this habitual state. The practice of habitual purgatives unhappily prevails most in the cases, where default of natural action arises from simple torpor of the intestinal canal. Yet these cases (so common among the higher classes of society, from sedentary life, deficient exercise, and other habits of society) ought especially to be exempt from the irritation of strong or frequent medicine. Dyspeptic symptoms, with increased torpor, are usually the immediate effect; while more serious disease frequently occurs as the sequel and consequence of a long-continued habit.

The Colon, perchance, cannot readily or quickly propel its contents, though the earlier stages of digestion are well and easily performed. To remedy this defect, it is goaded by the constant use of cathartics, which injuriously excite and fret the stomach and long tract of the bowels, through which they have to pass before reaching this part. Such habitual irritation of the

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mucous membrane, and of the intestinal glands, alters and depraves their secretions throughout the whole course of the alimentary canal, becoming thereby a further source of mischief and suffering to the patient. These disordered secretions are too often urged in proof of the need of further evacuation (an error sometimes arising from inexperience in the practitioner, sometimes from a much graver source); and thus the practice proceeds in a vicious circle of habit, from which the patient is rarely extricated without more or less of injury to his future health.[1]

It is not enough considered, either by physicians or patients, that a certain degree of distension of the alimentary canal by solid contents is even necessary to its healthy state. It is probable that this is more especially true as respects the colon and rectum. I cannot doubt, from observation, that these bowels, even if not actually assuming diseased state from constant irritation of the mucous membrane, are often much injured in their functions by the want of equable and sufficient distension, which the habit of purging implies. Distension by air, which is the alternative, produces various irritation and distress, and impedes in every way the proper peristaltic motions of the intestine.[2]

In effect of the causes of disorder cited above, the nutrition of the body generally suffers; the processes of digestion are imperfectly performed; the ingesta are hurried forward without the due amount of change and separation taking place; and there is usually decay of flesh and strength. The expressions of Celsus, quoted below, are common to all the ancient medical authorities, and founded on the unequivocal observation of facts.[3]

The effect of sudden and violent diarrhœa in depressing the vital powers is well known. Syncope is a frequent consequence; —death, when there is already great exhaustion, an occasional one. I have seen instances where a strong purgative, given directly after a severe and protracted operation, or after a shock to the nervous system from accident, has produced very urgent

[1] It is especially in this part of the treatment of dyspepsia, that Homœopathy gains advantage—not by its infinitesimal doses, but by forbearing from frequent and strong purgatives; and thus giving scope to that resumption of the natural actions of the body, which our common practice does not sufficiently confide in.

[2] *Το ὑποχωρεὸν εστω σκληρον· πληθος δε κατα τα εισιοντα.—Hippocrat. Prœdic.*

[3] Purgationes, ut interdum necessariæ sunt, sic, ubi frequentes sunt, periculum afferunt. Assuescit enim non ali corpus; et ob hoc infirmum erit; cum omnibus morbis obnoxia maximè infirmitas erit.

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danger by aggravating the tendency to collapse. These are extreme instances, but they form the index to others of more common occurrence. In cases where there is the habit of slighter, but constant, irritation by purgative medicines, the tendency of result is the same, though the immediate effects are less rapid and obvious. The extent of highly sensitive surface, forming the canal of the bowels, gives great scope to this influence; and few consequences can be stated more certain than the gradual undermining of the vital powers by the abuse in question.

Disordered action of the heart, whether depending on disease, or on mere irritability of the organ, is often much aggravated by the habitual use of purgatives. Very frequently I have known it produced by this cause in nervous or dyspeptic constitutions, where the relinquishment of the habit has been found the only effectual cure. Such effects are readily explained, not only from the flatulence and acrid secretions thereby produced in the stomach and bowels; but also from the unequal balance and sudden changes in the venous circulation of these viscera, which always attend purging, and which affect directly the regular transmission of blood to the right side of the heart;—and further, it may be, by an impression upon the nervous centres, producing that sudden collapse, which I have already noticed as common under any inordinate action of the bowels. A discreet judgment is the more needful here, because there are other cases where disturbance of the heart is the effect of congestion about the liver and other chylopoietic viscera; requiring large evacuation for its relief. Firmness of practice, also, is essential in these instances; the persons who thus suffer being such as are least capable of interpreting their own cases, or persisting in any principle of treatment.

Every physician must be familiar with the frequent confession of patients, even of those most wedded to the habit, that they feel better on those days when there is no action of the bowels. Of such avowal we are bound to take advantage, unless there are circumstances especially to prevent us. It is difficult, in truth, to find a footing for relief in cases of this kind. The notion of instant remedy clings pertinaciously to such patients, and the mind becomes morbidly engaged to the habit. I have sometimes been successful in checking it, by expressly enjoining an action only on alternate days, as needful to health; thereby

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giving a new course to the sensations, and to the imaginations which attend them. In regarding the extent to which this abuse exists, I am well persuaded that the judicious physician may do almost as much good by withdrawing medicines of this nature, as by their employment in cases of real need. And this, if it be a just opinion, is saying a great deal.

For while thus speaking as to the excessive use of purgatives in ordinary health,—or in dyspeptic cases, where their abuse is the greatest,—I would in nowise seek to depreciate their value, as the most powerful means we possess in the treatment of disease;—nor deny that modern practice has carried them into successful use (both by themselves, and in aid of the action of other remedies) where opposite principles of treatment had long unavailingly been employed.

In truth, there are cases where the bold and steady persistence in this method produces effects attainable in no other way. Such is especially the fact where the head is the part affected;—or the liver and other organs connected with the system of the vena porta;—or in certain cases of dropsical effusion, depending on disease of these organs, and where the kidneys refuse their office;—or again, where the body is disordered by certain morbid matters collected and circulating in the blood, the removal of which can thus only be speedily and sufficiently obtained. The latter case, of which I have spoken more at large elsewhere, is one of much importance in pathology.[1] I may describe it briefly here as that attested in practice by the very large and long-continued discharge of dark grumous matters, usually termed bile, and understood to come from accumulation in the liver; but which I doubt not to be secreted in great part from the membrane or glands of the intestines; and to be a gradual separation from the blood of matters noxious to the system; such as produce, by retention, various forms of active disease. Here, if natural evacuations do not occur, the free and adequate use of purgatives gives proof not less of the judgment than of the boldness of the practitioner.

In the foregoing cases (and others might be added to them) this vigor is sanctioned to an extent even beyond that to which it is carried in ordinary practice. For it is not paradoxical to affirm, that the practitioner who deals too familiarly with these

[1] See Chapters XVII and XXI.

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medicines in all cases, is less likely to use them boldly and sufficiently where the need is greatest. The accustomed habit of practice is carried on to the particular emergency, and due discrimination is wanting in both.

And again, with respect to the amount of dose, in those cases where there is express demand for purgatives for the relief of disease, it is a frequent error, and often arising from the very cause just cited, to reduce this too much; and to substitute small repeated quantities for those capable of more instant and powerful effect. With the exception of cases of very feeble vital power, where sudden changes of any kind involve risk, there is commonly less disturbance to the constitution from this decided practice, with fitting pauses between, than from the seemingly milder plan of small doses and daily repetition. And further it is to be noticed, that the large dose often produces action different in kind, as well as degree, from that of smaller amount; and is in this respect more beneficial in those instances where free evacuation is wanted from the system at large.

Yet, even in many forms of actual and dangerous disease, it must be admitted that there is excess in our employment of these medicines, and some justification of the reproach which English practice has incurred on this score. In cases of obstruction of the bowels, where the name seems to lend complete sanction to the treatment, we may affirm that it is often carried on too grossly and indiscriminately. If vomiting does not actually prevent it (and frequently indeed at the risk of bringing this on) purgative is hurried after purgative, with increase of dose, or adoption of more drastic medicines, at each successive step;—often with little thought as to the causes of obstruction,—with much danger of producing inflammation in the obstructed part or elsewhere,—and with no due allowance for the tendency which all parts have to resume a healthy action, if left in quiet.

I do not mean that purgatives should not be given in cases of obstructed bowels. But I am anxious to convey a caution, required, as I think, by the too uniform direction of practice in these instances; viz., that if there be distinct local pain, threatening inflammation in any part of the canal;—or much active irritation, with nausea and vomiting;— and if the first cathartic

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medicines, freely given, fail of success,—it behooves the physician well to consider whether he shall urge this treatment farther. There are doubtless cases where it is expedient to do so; but many others where the irritation of drastic purgatives hurries on the patient to danger of fatal result;— and this not unfrequently, where thorough quiet; abstinence equally from food and medicine; leeches and fomentations over tender parts of the abdomen; or still better, in many cases, leeches to the hemorrhoidal vessels; would have removed obstruction, and the danger consequent upon it.[1]

In these important cases of apprehended obstruction of the bowels, it is obvious, on the grounds just stated, that the first use of cathartic means should be the most vigorous, instead of the reverse. If the effect be obtained, it signifies little that the dose has somewhat exceeded the necessity. If otherwise, the chances of mischief by repeated and augmented irritation are spared, and we obtain the indications most to be desired for further treatment.

There can be no doubt of the fitness of using purgatives in the early stage of most fevers; though I believe it might be well in almost every case to let emetics have the precedence. But even in fevers there ought not to be daily purging, as is often practised; nor ought the use of such medicines to be continued needlessly or harshly, during the early periods of convalescence, when nature is seeking to right itself in all its functions, and can ill bear such interference. The same remarks apply still more explicitly to many diseases of local inflammation, where this treatment persevered in becomes a certain source of mischief to the

[1] The latter point of practice has not been sufficiently attended to; though more on the Continent than in England. I doubt not that in various cases of visceral inflammation, obstruction, or disease, much more might be attained by drawing away blood from the hemorrhoidal vessels, than by the methods of treatment generally in use. Looking to the connection of these vessels with the circulation through the liver and intestines, it is a strong presumption (and my experience justifies me in so stating it) that no given quantity of blood can be abstracted elsewhere, in cases of this nature, with equally good effect. The remedy particularly deserves trial in inflammation of the bowels, with obstruction. The influence of the state of the liver upon hemorrhoids affords presumption of what may be obtained, in disorders of this organ, by bleeding from the vessels of the rectum. In the following chapter (on Bleeding in affections of the Brain) I have spoken of this remedy, as applied especially to cases of cerebral or spinal disease.

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patient.[1] In inflammations of the chest, for example, as well as in other disorders of this part, cathartic medicines are often employed to a very injurious extent. They have no specific effect in such cases; and though the removal of vitiated secretions from the stomach and bowels is needful in aid of other treatment, and as preventing the access of fresh disorder, yet their retention for a while is generally much less hurtful than the irritation created in thus incessantly forcing them away.

It may further be affirmed (without recurring to any ancient theories of fever) that there is frequent injury arising from interference with the ordinary course of a disease, when this is so far definite as to mark a series of changes, connectedly taking place in the system. In the exanthematous fevers more particularly, the use of strong and repeated purgatives, prior to, or during, the eruptive part of the disease, is often a well-marked cause of evil—retarding or repelling the eruption, and transferring morbid actions to parts out of the ordinary course of the disorder. The experience of every observant physician will remind him of cases illustrating this fact. The vigor of practice, which is mischievous and out of place here, more fitly belongs to the time when an eruption is receding; and when morbid matters, the cause or product of the disease, are seeking egress from the system.

These considerations include some of the most important questions in pathology and practice; nor have they, as I think, been duly recognized in treatment of the diseases at the present time.[2] The knowledge where to abstain is at least as valuable as that where to act. To preserve the character of medicine as a science, each path must be kept open; and the practice be decided by a regard both to what is the natural or necessary course of symptoms, and to those new actions which it is in the power of medicine to produce and maintain.

Without embarrassing the question by the endless nomencla-

[1] *Εν τοισιν οξεσι παθεσιν ολιγαχις, και εν αρχησι τησι φαρμαχιησι χρεεσθαι,* is the expression of Hippocrates, in his aphorism on this subject.

[2] Even as respects the practice of bleeding in acute inflammations, there is something yet to be learnt as to the relative fitness of the remedy at different periods of the disorder. We may wholly put aside the limitation of Hippocrates to bleeding on the fourth day in inflammation of the lungs; and yet acknowledge that one time for such remedy is fitter and more effective, as regards the given quantity of blood removed, than another; and further, that there may be specific inflammations of different organs, in which the rule materially varies.

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ture of fevers, I would remark that the disorder, often epidemic, to which the term of gastric fever is fairly applied (where there is manifestly great disturbance, congestion, and depraved secretion of the chylopoietic organs without any symptoms of true inflammatory kind, but with tendency rather to a typhoid character), is, on my experience, the form of fever most benefited by the full use of purgatives; not merely in clearing away disordered secretions already in the bowels; but further, perhaps, by aiding their more speedy and effectual separation from the blood.

It will, I think, be found, that most physicians who have begun their practice with a large employment of active purgatives, abridge this more or less as their experience is matured by time. No physician, indeed, as I have before said, can rightly relinquish the benefits of this treatment, boldly and vigorously employed, in certain cases of disease; but the limitation of such vigor to actual disease is that which a wise experience will teach, as the general rule of practice. It is a rule, however, like all others in medicine, with many needful exceptions. One of the most frequent and reasonable of these regards the use of laxatives with direct reference to the *prevention of illness;* as in those cases where, from constitution or habits of life, there is a strong tendency to accumulations, morbid or otherwise, in the vascular system, or to various forms of deposit in the body. Here full purging, at certain intervals (and these may often most usefully be made periodical) is the best safeguard we have against contingent evil. The gouty temperament, when strongly marked, will occur as an instance in point. The acute attacks of this disorder, if not obviated, may at least be mitigated and rendered less frequent by such periodical evacuations, giving egress to some part of the material of the disease. But here, too, the same principle must be kept in mind, and forbearance be blended with our vigor; otherwise the prevention of one mischief becomes the source of many.

Nor can the need be doubted of aiding the natural action of the bowels in many cases where there is habitual costiveness, either from particular temperament or the casual condition of life. But I have already pointed out the great abuse by excess in this part of practice; and the importance of giving more scope than is usually done, to the natural powers of the organs

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in retrieving any such casualties. I advert to the subject again; as well to enforce this, as also to suggest the value in these cases of the direct combination of tonics with aperients; a form of prescription which might well be brought into more general use. In the greater number of instances of this kind, weakness in the proper propelling action of the bowels is the cause of costiveness; and in seeking to remove the effect by means which act through irritation only, we do but add to the mischief, from the invariable effect of irritation in lessening power.

Here, then, the tonic, conjoined with the aperient, generally enforces its action, without weakening the organs. These in fact are the cases in which, if there be no irritation of the mucous membrane, bark will itself often act as a laxative. I have known many instances where calomel, colocynth, and gamboge, in large doses, have had little other effect than that of injurious irritation; but where a few drachms of infusion of senna, with decoction of bark, have been amply sufficient in habitually producing the desired action. The benefit obtained in certain disorders from the combination of laxatives with the preparations of iron, though depending probably on other conditions, is well known to every physician.

Such methods of practice are of more especial value in those languid and strumous habits in which good digestion is so essential to the maintenance of health, but where the digestive organs themselves partake in the general debility of the system. If these organs be further weakened by an irritation habitually repeated, without any countervailing aid to their functions, not only is nutrition impeded in such cases, but the foundation often laid for more active and lasting disease.

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CHAPTER XXIII.

ON BLEEDING IN AFFECTIONS OF THE BRAIN.

THOUGH the form and argument of this Chapter are retained as they stood in the former editions, the errors in practice to which it relates are certainly of late years lessened in amount, and I have reason to indulge the belief of having in some part contributed to this effect.[1] The question, however, may yet be

[1] The change, alluded to above, must in part be associated with the present more limited employment of bleeding as a general remedy. This very important modification (for I would not willingly call it *a fashion*) has chiefly occurred within the last twenty-five years. It is equally difficult to prove, or to disprove, the opinion entertained by some physicians, that it depends upon, and is justified by, an alteration in the general type and character of disease during this period. It may well indeed be, that the frequent injurious, or even fatal, effect of bleeding in the earlier epidemic influenzas of our own time, before the adynamic character of the disorder was sufficiently known, has had influence in limiting the use of this powerful antiphlogistic remedy, even in cases where it was before deemed of the first necessity. Nor can it be reasonably doubted, that the continued fevers occurring during the last twenty years, have had more of typhoid character, and borne less depletion than heretofore. But the ulterior question still remains, whether we may suppose an *epidemic constitution* to extend over this later period of time, disposing all diseases to assume this low type; and hence prohibiting or abridging the use of all such means as tend to abate the vital power, and of bleeding as the most powerful of them? This is a question which descends to us even from the days of Hippocrates; though generally applied rather to the constitution of a particular year or season, than to a series of years, as in the case before us. I cannot pursue the subject here; but it well merits attention as a part of medical philosophy.

It is curiously instructive in medical history, to contrast the large and various use of bleeding at other periods with that customary at the present day. We must feel that Le Sage has in no way colored his Dr. Sangrado too highly, when reading the *Lettres de feu M. Guy Patin, Docteur de Medecine de la Faculté de Paris;*—one of the most curious, as well as voluminous, collections of letters that exist; and prolific of materials for the political, as well as social and medical history of the time. The record they give of the struggle between the disciples of the *Lancet* and those of the *Antimonial school* is eminently amusing.

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urged, whether the practice of depletion by bleeding is not still too general and indiscriminate in affections of the brain, and especially in the different forms of paralysis? I believe that the soundest medical experience will warrant the opinion that it is so. The vague conception that all these disorders depend upon some inflammation or pressure, which it is needful to remove, too much pervades and directs the practice in them: and if the seizure be one of sudden kind, this method of treatment is often pursued with an urgent and dangerous activity. Little heed is taken of the many cases where the symptoms depend upon irritation alone—or on loss of nervous power—or on deficient circulation of the blood within the brain—or on altered qualities of this blood—or, it may be, on morbid changes in the nervous substance itself. Theory might suggest that in some of these various cases, the loss of blood would lead to mischief. Experience undoubtedly proves it; and there is cause to believe that this mischief, even in its abated form, is still neither infrequent nor small in amount.

It is certain indeed that there is a state of brain, best perhaps represented to us in its general effect of diminished nervous power, which tends to produce sometimes spasmodic seizures; sometimes delirious or maniacal affections; sometimes palsy of different parts of the body;—these effects being in no way obviated by depletion, but rather increased by all such means; while they are relieved by remedies which tend to excite the energy of the sensorium, and to augment the general power. I have known cases of this kind where bleeding has immediately been followed by convulsions of epileptic character; occasionally by amaurosis or deafness; more frequently still by rambling delirium; and where wine or other cordials have as speedily abated these tendencies. The fevers of typhoid type, or those which are called nervous (in default of a better name) afford constant examples of delirium depending on this peculiar state of the sensorium. In cases of large and sudden hemorrhage (as in that of the uterus after childbirth) the whole system is often put into a state of seeming excitement; and the head in particular affected with acute pain, throbbing, and vertigo; which symptoms sometimes continue for a considerable time, and are best removed by such means as gradually and equably restore the circulation and nervous power. Delirium, preceded generally by vertigo, is

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known to be an effect of extreme starvation, without other obvious disease; as frequently recorded in the narrative of shipwrecks, &c. The condition of the patient in *Delirium tremens*, on whatever proximate cause this may depend, is one of the most marked instances of that state of brain which any large depletion might hurry on to fatal result. Here indeed the treatment is not often mistaken; but in many cases essentially the same, though less distinct in symptoms, and less obvious in their origin, the mischief done by large bleeding is long continued, and sometimes of irreparable kind.

In the valuable experiments of Sir Astley Cooper (Guy's Hospital Reports, No. III), the tying of the two vertebral arteries, which have seemingly a more important relation than the carotids to the cerebral circulation, brought on various forms of paralytic as well as spasmodic affection. Here we have direct proof that the brain, suddenly and largely deprived of blood, becomes liable to conditions apparently the same as those which we so often employ bleeding to obviate or relieve. They may be really different in many cases, and the facts show them to be so. But where we have this source of ambiguity, it behooves that our practice be carefully guided by a reference to the constitution, habits, and age of the patient, as well as to the first obvious effects of the treatment pursued. I have known many instances where bleeding has been repeated, to remove the very symptoms which it was next to certain it had been the means of bringing on.

In the experiments just referred to, sudden privation of blood produced a state of the brain which doubtless exists in effect of other causes of slower operation; manifesting itself in every grade of deviation from the healthy state. It would be difficult to define it by name; but presumably it has close relation to that already mentioned, where there is a deficient or altered nervous energy;—testified in slighter cases chiefly by its influence on the mind and animal spirits; in those more severe, by the tendency produced to convulsive and paralytic affections. This temperament, doubtless depending on a physical cause in the organ, will be familiar to all who have attended to this class of diseases. It appears to have much connection in its nature with some of the changes which occur in the brain in old age; and possibly from similar causes as respects the supply of blood

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to this organ. Anxiety and distress of mind long continued, or even the habit of overstrained intellectual labor, produce a similar state; and furnish much reason to the physician why he should exercise singular caution in all such cases. Certain it is that bleeding unduly practised in habits of this kind—and especially when the time of youth is past—depresses the nervous system, and through it, more or less, all the functions of the body; producing for a time a sort of premature approach to old age throughout the whole being. I have notes of some remarkable cases to this effect, in which the influence of the cause was unequivocally continued for a long period, passing even into years after the original occurrence.

In infancy, also, when the brain and nervous system are more easily and critically affected by causes of excessive or deficient excitement, effects of the same kind follow great depletion, however produced. These, by a fatal error, are often mistaken for proofs of inordinate action, or of pressure; and a practice adopted of which it is difficult to recall the consequences, when once incurred.[1]

Even in those cases of cerebral disorder where the tendency to coma might give stronger presumption of pressure, I have sometimes had the most direct cause to believe that large bleeding induced paralytic attacks, which might otherwise have been spared.[2] These are instances in which absolute proof can scarcely be attained; but where the presumptive evidence may be such as nearly to approach to it. Two or three examples have occurred to me, in which—looking at the circumstances before and after, as well as at the immediate concurrence in time of the bleeding and paralytic seizure—it could hardly be doubted that they stood to each other in the relation of cause and effect. The

[1] This important point in practice has been explicitly noted by some of our best medical writers; but experience shows that it is still not sufficiently enforced.

[2] In using the term *Cerebral* here, as throughout this chapter, I have not thought it needful to advert to the distinction now established between the brain proper, and the system of the medulla-oblongata. The relation of these respective parts in their morbid states is so complex and close, as far as our present means of diagnosis go, that, even in reasoning upon points of medical practice, it is necessary if not expedient, to consider them together. Even if we had more certain methods of distinguishing them, we have yet no well-determined rule of practice to adapt to this distinction.

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existence of such instances, or even the suspicion of them, may well warrant a singular caution in this part of practice.

The state of Coma is indeed a very ambiguous one, as respects our knowledge of its proximate cause. In the ordinary use of the term, the notion of pressure is associated with it by most practitioners; and this even in the cases where it seems but as an excess of the condition of sleep. But it is familiar to us also as a concomitant and token of the last stage of debility; and is often expressly induced by depletion and other depressing causes, as well as by those which are known to produce direct pressure on the brain. Even in children we have express instances of a state having all the characters of coma; but which is proved by the precursory causes, as well as by the effects of medical treatment, to depend on general feebleness of circulation and deficiency of nervous power. There is reason, indeed, to presume that the two states just alluded to are really distinct; and that the former is much more nearly akin to the condition of syncope than that of pressure: or however related, as respects the nervous substance itself, so different in the operation of the causes producing it, as to require a method of treatment altogether opposite. Here, then, the governance of names must be put aside; and the more sedulously, from that close resemblance of symptoms which makes the utmost discretion of the physician necessary, rightly to direct his practice for their relief. Nor can any rules be given, at once general and exact enough, to supersede the particular judgment in each case.

Vertigo, in all its degrees, is another symptom which often conveys alarm, from the idea of pressure on the brain, and leads to immediate depletion under this view. In some cases the inference and practice are just and beneficial. But there are others (and these of very frequent occurrence) where vertigo is the effect of causes wholly remote from fulness or over-action; being in fact the approach towards syncope, and a proof of exhaustion of the nervous power. Here we require, not bleeding, but cordials, to restore the due action of the heart and brain; and a wrong practice may lead into serious danger. As in the instance of coma, just mentioned, we must presume a wide distinction between states requiring a treatment so different, though the seeming resemblance of symptoms has led to the same name for both. The giddiness and confusion of senses,

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which precede a fainting-fit, must clearly be separated from symptoms which depend on vascular excitement of the brain. The frequent occurrence of vertigo, in apparent good health, as well as under actual or threatened disease, is a reason for sedulously noting this distinction; which may generally be done by observation of other symptoms, or of the cause especially acting on the body at the time.

The difficulty of getting a correct nomenclature for morbid sensations applies peculiarly indeed to the head; of which the ambiguous use of the terms *light* and *heavy head* may be taken as a familiar example. It must be owned, however, that the names here are not much more vague than the knowledge they represent. That state in which the head feels as if without weight or substance, common though it be, has not yet received any physical explanation. Many of the varieties of headache are as little defined by our knowledge of their cause, as by the terms used in describing them; though probably the greater number depend on different conditions of the circulation through the brain. Of these conditions that of congestion or retarded circulation, varying in the degree of vascular pressure it produces, and in the vessels in which it occurs, is presumedly the most frequent.[1]

Among other symptoms which are still to be considered doubtful, as representing states of the brain, I may mention the dilatation of the pupil, and susceptibility of the retina. Direct observations show that these conditions depend for their degree upon different causes; and accordingly we find, as a pathological fact, that, in cases of compression, concussion, or other disorder of the brain, we may have either contraction of the pupil, and a very sensitive state of the retina, or the reverse of this, without our being able securely to predict either of these states. Sometimes, too, they pass into each other; and each may occur in high degree, yet disappear without fatal or even mischievous consequence. Inequality in the pupils of the two eyes is perhaps to be taken as a more certain proof of some organic injury. But here also we can rarely affirm whether the dilated pupil concurs

[1] In regard to the cerebral circulation, and the effects of changes of vascular pressure within the cranium on the functions of the brain, the reader cannot do better than consult the valuable treatise by Dr. Burrows, "On Disorders of the Cerebral Circulation," published a few years ago.

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with the side affected or not; and other evidence is required to determine our practice as regards this distinction.

I mention these various sources of ambiguity, not to inculcate distrust of all practice in such cases; but merely to hold out caution against that indiscriminate principle of treatment which finds pressure in every altered condition of the brain; and motive for bleeding in every transient irritation of parts, still but imperfectly known to us in their functions and various diseases.

Even were the tendency to paralytic seizures as generally lessened by bleeding as common practice would imply, it does not thence follow that abstraction of blood from the brain should be needful or desirable, in immediate sequel to such attack. In many cases it is undoubtedly otherwise. The paralysis, when depending on apoplexy, with extravasation of blood or serum, or on other cause of continued pressure, may come on by degrees, and admit of relief in its progress by emptying the vessels of the head. But often it occurs as an instant shock to a portion of the brain or spinal marrow, without any proof of extravasation or obvious cause of pressure;—the shock itself being of momentary duration, though leaving lasting effects on the parts of the nervous system thus affected. In these cases, and they are frequent, the physical causes of the change are little known to us. There are reasons for supposing that the nervous substance itself is often primarily affected. We have certainly no sufficient proof of mere pressure from fulness of vessels being concerned, to warrant large bleeding, especially after the stroke of palsy has actually occurred. The degree of coma, attending and following these seizures, is not alone sufficient cause for the practice; and will usually subside without it, where the original attack is not such as to endanger life.[1]

[1] The question as to what constitutes the cure of paralysis is not less difficult than that as to the causes of original seizure; our ignorance of the structure involved forming the difficulty in each case. The medullary fibres are presumably the parts immediately affected;—but whether by effusion between them, which is removed by absorption; or by any breach of continuity which is repaired; or by conditions yet more remote and subtle, we are unable to affirm. From the operation of causes affecting the brain alone, it may be inferred that in hemiplegia the morbid change takes place at or near the origin of the motor fibres; but the evidence as to other kinds of paralysis would seem to show that this is not a necessary part of the phenomenon.

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Looking, indeed, to the magnitude of the event which has occurred between, common reason would suggest a doubt whether the same treatment can be desirable immediately *before*, and *after* a stroke of palsy. I do not mean to give this the weight of an argument. From the nature of the circumstances, it is extremely difficult to bring unequivocal proof on the subject; but there is much cause to believe that the practice of bleeding in the latter case is often injuriously pursued. The risk, I believe, will generally be less from waiting a certain time—to observe the effect of what has occurred, upon the circulation, the breathing, and the sensibility—than from hastily taking away blood, at the moment of a great shock to the brain, and before we can rightly appreciate its consequences. This effect upon the principal functions of life, gives us in fact the best information we can have in guidance of further practice. But we forfeit all such evidence under the disturbance any large depletion makes in the system, and particularly in the organs upon which these functions depend. The importance of this consideration may be readily understood. It is a point constantly brought before us in practice.

Even where evidence is obtained of the fitness of bleeding soon after one paralytic attack, for the prevention of another, the question still remains as to the manner of this—whether by copious depletion at once, or by smaller bleedings, repeated as observation may suggest. And this question the practitioner, while prepared for boldness in all fit and urgent cases, is bound always to keep before him; seeing especially that any great excess in the remedy may hurry on the very mischief it is sought to prevent. I believe that in most cases the latter method is to be preferred. It accords better with the state of our knowledge of these disorders, involves no irretrievable step, and in its progress affords the information most requisite to decide how far it should be carried into effect. Paralytic cases there doubtless are, of such nature, that a few ounces of blood taken away at regular intervals will ward off a recurrence of the attack, which any large and sudden depletion would probably hurry on. The proof here can seldom be explicit, but the presumption is one I have often been led to entertain.

These remarks, and the cautions they suggest, are familiar to many, and to such needless. But I feel assured, from what I

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have seen, that they ought to be carried further into general practice. The use of the lancet is easy, and gives a show of activity in the practitioner at moments when there appears peculiar need of this promptitude. Current opinions and prejudices are wholly on the side of bleeding; and the complexity and danger of the cases tend to obscure the results of the treatment pursued. The physician needs all his firmness to decline a practice thus called for; where the event is so doubtful, and where death may be charged upon his presumed feebleness or neglect.

While making these remarks, however, I must be understood as recognizing, in the fullest sense, the value and need of this remedy, promptly and vigorously used, in various cerebral diseases, or in prevention of such, where well-marked symptoms lead to their anticipation. And I dwell upon this the more earnestly, lest, while merely inculcating a cautious discrimination as to the cases for its use, I may seem to be seeking reasons against the practice altogether. In active inflammation of the brain or its membranes—in many states of pressure from congestion in the head, topical or general, without inflammation—in cases where extravasation may be presumed to be going on, which it is the object speedily to suspend—and even in other conditions of cerebral irritation less definite than these, we have no method of treatment equally effectual; and safety often depends solely on the speed and sufficiency of its employment. But almost in the same ratio with the necessity of the remedy in these cases, is the importance of refraining from depletion in other instances, which can only be distinguished from the former by a careful observation of all the symptoms, and of the probable causes in which they have their origin. And, in the right direction of diagnosis and treatment here, we have the best proof that the mind of the practitioner is equal to the most difficult exigencies of his profession.

There are one or two lesser points in the treatment of affections of the brain, and connected with the circulation through this organ, which deserve more discrimination than they usually obtain. One of these is the general use of ice, or other cold applications, to the head. Any influence of cold, through its effect on the capillary vessels of the scalp, is at least ambiguous in such cases; and though its direct sedative effects on the

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nervous system may afford more certain warranty for the practice, yet these require to be distinguished and watched over in their progress. For even where relief is distinctly obtained from cold suddenly applied to the head (as after intoxication), it by no means follows that this application, long continued, will produce or maintain a like benefit. It is well known in various parts of practice that the fact is otherwise; and that the effect of cold upon the circulation and nervous system is sometimes even reversed, according to the manner and degree of its application.

Or allowing, as I readily do, that there may be a distinct sedative effect from cold, beneficial in cases of inflammation of the brain or its membranes, will this equally apply to apoplectic cases, where pressure occurs from fulness of vessels or extravasation? If the relief to some kinds of headache be alleged, it must be remembered that other headaches are increased by this means. The same disparity doubtless exists as to the more serious affections of this organ, requiring discrimination wherever we can exercise it. Unfortunately these are cases in which patients themselves can generally render least aid. Sometimes they are manifestly uneasy under the application of cold; and this feeling, whenever it can be ascertained, is far better than any other test. It is one of the cases where a patient may reasonably be admitted to judge for himself.

I believe that patients laboring under affections of the brain are too indiscriminately forced to a raised position of the head, with the view of lessening the flow of blood or congestion there. Such effect it undoubtedly has: and in many, perhaps most, of these cases it is a prudent precaution. Sometimes, however, it is employed without any good to compensate for the discomfort thereby produced. And further, there are many cases, like those already alluded to, where the diminished energy of the brain seems to require a full and equable circulation through it; and where the symptoms are aggravated by the compulsion of a raised posture. The effect of sudden recumbency in relieving faintness is a familiar illustration of this; and conversely, the vertigo which frequently occurs, especially in a weakened state of body, upon suddenly rising from a couch. Headache moreover in some instances is brought on, or much augmented, by the upright posture; and subsides when this is changed. In

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general we may gather from the patient which position of the head is best:—or the color of the face, the state of breathing, and the action of the heart will suffice to tell it, if he be unable to express his own sensations. Perhaps altogether the respiration is the most certain test we can employ in the absence of this expression.

Bleeding by leeches from the hemorrhoidal vessels might be much more frequently employed than it is, in affections of the brain, as well as in those of the spinal cord. I know no mode in which a given quantity of blood can be removed with equal effect in such cases, when depletion is required. It may be difficult to give strict anatomical reasons why this should be so: but what we know on the curious subject of the changes of balance in circulation, will at least furnish illustration of it. Nor can I doubt that something here is due to that peculiar relation between the blood in the portal circulation, and the functions and diseases of the brain, of which experience affords so many remarkable proofs. For the practice itself we have especial argument, in the frequent alternation of bleeding hemorrhoids, with headaches and other graver affections of the head; and also in the serious effects which sometimes ensue upon the suspension of such discharge, after it has become habitual to the system.

In the foregoing remarks, particularly those which relate to bleeding in affections of the brain, I have spoken of these disorders in a way which may seem too general and vague for practical purposes. In explanation of this, I would make one or two remarks. In the first place, I think the object likely to be best fulfilled, of deterring from indiscriminate bleeding, by broadly pointing out the great practical disparity of certain conditions of the brain, which have some appearances in common; and which even produce (though doubtless in different way) certain similar effects upon other parts of the body. The attention of the practitioner, if well awake to this distinction, will rescue him from a blind adherence to the one method of instant treatment, so generally in use. It will further direct him to those special indications—whether derived from the age and general habit of the patient; from the action of the heart and arteries; from the respiration; or from change in the functions

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of the brain itself—which may give better and safer guidance to his practice.

Secondly, while fully admitting the value of what has been written on diseases of the brain, and the great advances recently made to their better classification, I can scarcely think the latter object to be yet duly attained.[1] This remark applies alike to the diseases depending on inflammation of the brain and its membranes; on altered circulation in the organ from other causes; or on extravasation or effusion within the cavity of the cranium. Still more expressly does it apply to lesions, general or partial, of the nervous substance itself. Here the difficulties of exact observation, and ignorance of the functions of particular parts, have hitherto precluded any such intimate knowledge as might furnish a basis for classification and treatment; and we are compelled rather to look to changes in vital condition, than to actual pathological states, in reasoning on the subject.[2] That there are diseased conditions of the nervous substance, more numerous and varied than those hitherto described, may well be admitted both from analogy, and from notice of effects not easily referrible to any other cause. Nor can it be doubted that many of these will hereafter become known to us; the path for such researches being widely open, and the methods and instruments of inquiry much better fitted for success than at any former time.

Meanwhile, in this yet imperfect arrangement of cerebral disorders, there is cause for treating of bleeding as a general

[1] It is impossible not to advert here to the work of Dr. Abercrombie, which has done so much towards a right arrangement of these difficult and important diseases. Also to Dr. Bright's Treatise on Diseases of the Brain and Nervous System (vol. ii, of his Medical Reports), in which a very good basis of classification is proposed and followed. And, further, to the Memoir on Injuries of the Brain and its Membranes, by Sir B. Brodie, in vol. xiv of the Med. Chirurg. Transactions. I have already alluded to Dr. Burrows's work on the Cerebral Circulation.

[2] I may instance the question, still undecided, whether the state of *ramollissement* of the brain can ever occur without preceding inflammation. Dr. Abercrombie's opinion, that there are two distinct causes of this state, is probably that nearest the truth; and the recent microscopical researches of Professor Gluge (Archives de la Médecine Belge, No. I) would seem to confirm this opinion. The difficulty of obtaining unequivocal proof in these cases is hardly to be understood by those who do not know them on experience. The diagnosis in lesions of the brain is not aided by those external physical signs which have lately obtained such invaluable application to diseases of the chest.

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question of practice, even without express reference to the particular forms of cerebral disease. And from the difficulty as well as importance of this question, it is better perhaps that the medical man should have it constantly before him in this light, than that his judgment should be submitted to rules and nosological distinctions, the application of which to actual disease involves almost as great risk of error, as he can incur in proceeding without these technical aids.

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CHAPTER XXIV.

ON THE USE OF EMETICS.

Among the changes which time and fashion impose upon the treatment of disease, Emetics as a remedy have fallen into comparative disuse in English practice. It is no longer now, as heretofore, one of the first questions before the mind of the practitioner, when called to prescribe for a disorder, whether the stomach should not be immediately thus relieved. The suggestion of the patient himself is for the most part needed, or some other equally express indication, to lead to their use. I am speaking here only of a general alteration that has occurred. The exceptions to it are of course numerous; but the truth of the fact so stated will probably not be denied.

The cause of this change may chiefly be found in that larger and more various employment of purgatives, which forms the character of our modern practice. To what extent is the alteration of method a beneficial one? I believe a fair consideration will justify the opinion that in several respects the change has been carried much too far. Vomiting, especially when brought on at the outset of fevers and many other diseases, produces effects of a kind, and with a speed, which no purgatives alone can equally obtain. The action of Emetics is not merely valuable in unloading the stomach and upper part of the alimentary canal; but it is certain that their influence goes far beyond this point; and that other parts of the body, even the most remote and different in structure, are powerfully acted upon. The emetic, moreover, if rightly used, is beneficial not merely in its effect of removing morbid matters from the body; but often even by the nausea attending its operation, and by the muscular effort and struggle it induces, in the act of vomiting; a combination of advantages belonging to few other remedies.

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The experiments of Magendie seem to have proved that the stomach itself is passive in vomiting; and that the action is due to convulsive movements of the diaphragm or abdominal muscles; brought on, not solely by emetic or other irritating substances in the stomach, but by similar agents introduced directly into the blood; as well as by certain states of the brain and nervous system. As I have in view only the practical application of the remedy, I need not advert to these points in physiology, further than to remark, that they illustrate much more satisfactorily the various causes of sickness, and the effects of Emetics, than the old notion that the stomach alone was concerned in this action. There are still points of question as to the proximate causes of vomiting; even such as relate to the state of the diaphragm in the act, and in what particular way it is connected with the function of respiration. Something also may be learned of the manner in which particular irritations produce sickness, by reference to reflex nervous actions through the spinal cord. But, for the reason just given, I merely allude to these topics of inquiry.

It is unnecessary to say much of the effects of vomiting as a remedy upon the stomach itself. The relief got by rejecting undigested food, or morbid ingesta of other kinds, is not, however, to be regarded as the only benefit thus obtained. The secretions from the mucous membrane lining this organ are frequently such in kind and quantity, that their removal becomes necessary to all further treatment; and accordingly we find that instant good is often derived from Emetics, where these matters alone are discharged. While still left on the stomach, all other treatment by internal remedies is injurious or unavailing;—a point not sufficiently regarded in ordinary practice, where the failure of one medicine is too often made the prelude to another trial of similar kind. It may be that Emetics have influence also in changing the nature of the secretions from this organ, either by inducing the state of nausea, or by the act of vomiting; each, as it would seem, capable of this effect, though perhaps in different manner.

Much caution, however, is necessary as to the use of Emetics in another state of the stomach and alimentary canal where the secretions from the lining membranes are almost wholly suspended; and where there is general gastric irritation, tending to inflammatory action, indicated by redness and dryness of the

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tongue, fauces, and throat; and often by tenderness over the region of the stomach itself. These indeed are cases where the effort to vomit is often a symptom, and a very distressing one, of the disorder; and where, of course, Emetics are interdicted by the most obvious circumstances present.

The value of Emetics in the treatment of Cynanche tonsillaris, though understood as matter of medical precept, is not equally regarded in practice. Nor, indeed, as a general fact, do we sufficiently keep in view the close relation between the several parts of the internal fauces and the membranes of the œsophagus and stomach; and the frequent and singular translations of morbid action which take place along this continuity of surface.

Many more exact interpretations of disease, as I have elsewhere remarked, might be derived from looking to this connection through contiguous structure and function, than from the minute division and nomenclature which are formed upon the mere locality of symptoms: and all the indications of treatment would in the same proportion become more exact, as instanced in the particular case to which I am now referring.

The liver, and the whole system of the portal circulation, are singularly under the beneficial influence of Emetics as a remedy. Congestion, so prone to occur in this part of the body at the commencement of most fevers, as well as in other diseases, is with greatest ease lessened or removed by vomiting;—the passage of bile into the bowels rendered more free;—other secretions promoted or restored;—purging often obtained without further aid;—and a general state of all these viscera induced, peculiarly tending to diminish febrile action, and especially in those cases where it arises directly from gastric disorder.

In most of the slighter cases of Jaundice with which we have to deal, Emetics will be found much more capable of speedily restoring the passage of bile into the bowels than any other means. The instances, again, are numerous (including particularly those fevers of warm climates, which, however variously designated, have all such close relation to the functions of the liver and the circulation through the chylopoietic viscera) where the direct combination of an active emetic and purgative medicine in the outset of disease, with repetition of the same means when needful, has effect in subduing the violence of disorder beyond any other remedy. This combination is not sufficiently

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employed in ordinary practice, in the many cases where speedy disgorgement is required of the part of the system just referred to.

The effect of Emetics upon the circulation is a complicated one, including, as it does, both nausea and the mechanical effect of vomiting; each capable of influencing greatly the heart's action, and the general movements of the blood. From both may much benefit be obtained; though under different mode of use according to the nature of the case. When the circulation is morbidly excited, nausea may be longer sustained; —where there is congestion about the heart and great vessels, vomiting ought more speedily to be brought on. Nor is there cause for the degree of apprehension which exists as to the rupture of bloodvessels, or pressure upon the head, from this effort. Though cases of such kind have doubtless occurred, they are comparatively very rare, and generally depend upon causes peculiar to the individual. It is singular indeed to what extent, and with what violence, vomiting may take place, without producing any injury of this nature. Sea-sickness, and that attending every stage of pregnancy, are evidence of the fact; and it is important this inference should be kept in mind, that there may not be undue discouragement to the use of the remedy in many cases where it is of singular value.[1]

The influence of Emetics upon the nervous system is also complicated; and one in which it is not easy to separate the direct from the indirect effects. It is unquestionably powerful, and capable of being very beneficially applied. The circumstances, not yet wholly understood, which give such peculiar sensibility to the region of the stomach; and so closely associate it, not only with all others of the vital organs, but also with the functions of animal life, render the several conditions of this part most important to the well-being of the whole system. No illustration of this is needful to those who have felt the influence, both upon mind and body, of the sensations arising from disorders having this seat, even such as are casual and temporary

[1] I may mention in further proof the common use of Emetics among the workmen in coal-mines, for the relief of the state brought on by breathing fixed air in excess, as well as after being subjected to fire-damp. I have had recent occasion of learning this fact in visiting the mines at Whitehaven. It might be supposed in these cases, if in any, that congestion in the vessels of the brain was to be avoided. Yet if injury be thus produced, it is seemingly of much less import than gained by renewing the action of the diaphragm and restoring respiration.

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in kind.[1] Here the action of Emetics is well marked, and often of singular avail; and from hence their effects are largely diffused over other parts of the nervous system. Some part of the good they produce, when given at the outset of fevers, may depend on this circumstance, in concurrence with the others already mentioned.

In illustration of the benefit to be obtained from Emetics in this stage of disease, I would refer again to epidemic influenzas, which have so repeatedly prevailed of late years. I have already stated my conviction that no remedy is equally sure and expedient, at the commencement of the disorder, in diminishing the severity of the attack, or apparently, when slight, in suspending it altogether. In the progress of the malady, too, there is often benefit from the repetition of the same means, and especially in the case of children, where the want of due relief by expectoration is best thus supplied. Here, as in other fevers tending to an intermittent or typhoid type, it forms the best and most speedy preparation for the use of bark—a fact well worthy of being kept in mind by those who travel through districts subject to malaria, and the febrile disorders consequent upon it. I name this instance of the epidemic influenzas as one where the comparative disuse of Emetics has certainly been injurious to the general success of our practice.

The mention of Emetics as aiding expectoration brings us to one of the most important uses of this remedy. Though yet insufficiently employed for the express object, their effect in relieving the chest, when the bronchial cells and tubes are gorged with mucus, is scarcely less beneficial than their action in unloading the biliary system. They may justly be reckoned the most powerful expectorants we possess; speedy in effect, and often complete in relief. In bronchitis and other cases, where, from accumulation of mucus in the air-passages, the breathing and pulmonary circulation are greatly oppressed, and the patient under much suffering, the change thus produced is sometimes surprising in degree, and such as we can obtain in no other way.

[1] The fitness of the term of *foyer epigastrique*, in expression of a fact respecting this particular part of the body, has been made the subject of some controversy. Bichat denies its propriety as applied to any one spot; but, in connection with his hypothesis that the vital organs are the proper and sole seat of the passions (an hypothesis which has foundation in language alone), he makes a sort of admission of the phrase in question as expressing a central influence of these organs.

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Their value in croup is well attested, by the concurrence of all modern experience with that of the excellent physician who first enforced this treatment.

I have already commented on the apprehension of hemorrhage, or pressure on the head, which checks one important application of Emetics. In the case now before us, their use is often abandoned from the like fear of making undue pressure on the vessels, or producing suffocation; an apprehension unwarranted by fact, and which the most common experience might correct. The converse of this is nearer the truth; and forms, indeed, the especial value of the remedy in the instances alluded to. Even in cases of actual inflammation of the lungs I believe the risk of their use to be exaggerated, though undoubtedly more watchfulness is here required.

There is the greater cause for referring to this application of Emetics, from our vague and imperfect views as to the whole class of expectorant remedies. Scarcely, indeed, is the term defined in its ordinary use in practice. It is less doubtful whether the expectorant is a medicine which promotes the secretion of mucus from the bronchial surfaces—or facilitates, after being formed, its removal from the chest—or combines both these effects in its single power. The natural result is that of rendering practice almost equally vague on these points. And, though there is less liability to dangerous error here than in many other classes of remedies, yet it is obviously important to gain more distinct views than those currently received.

The Emetic is probably the only agent which both promotes secretion, and directly discharges the matter secreted; the latter action being chiefly a mechanical effect of the effort of vomiting induced. There is reason to believe that no one of the medicines termed Expectorants can act in freeing the chest from mucus in the air-passages, unless they be so given as to produce vomiting, or to bring on cough by irritating the membranes;—or unless they increase or alter what is secreted, so as in this manner to excite cough, and render it more effectual for expectoration. Their influence upon secretion is indeed the circumstance we must chiefly regard among medicines of this class; and here, again, their effects and relative value are very ill defined. It is probable that the expectorants, so termed, which act as emetics in larger doses, are principally of avail in changing

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the secretions when used in such manner as to keep up a certain degree of nausea. This state has manifestly much influence in relaxing the exhalant and secreting vessels; and though I do not venture to affirm it, where proof is difficult, I believe it to be chiefly in this way that the medicines in question have gained their present reputation in practice.[1]

The best aid to expectoration in ordinary cases, and where no inflammatory symptoms exist, is that of taking a moderate quantity of some warm food, either liquid or what is lightest of digestion, on the stomach. The effect of this is often felt immediately in augmenting and loosening the secretion of the mucous glands about the throat and fauces; while at the same time more assistance is given to the effort of coughing, by which the mucus is extricated from the air-passages.

The question as to the proximate cause or seat of the sensation which we term *nausea*, and its relation to the act of vomiting, is in all respects a curious and difficult one. If any true explanation is found hereafter, it will probably be through the results obtained by Magendie and others, to which I have already alluded.

Connected with this subject is the alleged effect of Emetics as a remedy in the early stages of pulmonary consumption; an opinion held by many eminent physicians from an early period down to the present day; and which has gained rather than lost weight by recent inquiries on the subject. The researches of Sir R. Carswell into the origin and seat of tuberculous deposits afford a clearer notion how Emetics may act, by removing or preventing the growth of tubercle on the membrane of the bronchial cells. It is easy to understand that any means which can promote the natural secretion into these cells, render their contents more liquid and easy of removal, and aid in actually procuring it, may be of singular advantage;—especially in that earlier part of the disease, where the presumption exists that tuberculous deposits are only beginning to take place. The action of Emetics reaches further towards these several objects than

[1] The whole class of Expectorants, however divided and defined, needs revision, as do so many other parts of the Materia Medica. The progress of medicine, as a science, requires that we should not bind ourselves too implicitly to old tables and formulæ; which have their origin in doubtful sources, and gain authority chiefly by long transmission from one book or lecture to another.

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that of any other remedy;—it is compatible with every other part of treatment required;—and under regulation of their use, and with due regard to any acute inflammatory states which may occur in the progress of the disease, I believe them to be among the safest and most probable means yet suggested for the relief of incipient phthisis.

Unfortunately this must still be stated more on speculation than on certain experience. A few Continental physicians have made systematic use of the remedy; but in England its employment with this object seems to have been very partial, though enforced by medical authorities which claim much respect. The difficulties, indeed, which oppose themselves to the treatment, especially among the higher classes, are not easily overcome. The remedy, from various associations, is regarded as more formidable than is really the case;—the present habit of practice is adverse to it; and, further, the suggestion of the treatment comes at a time when fears may hardly yet be awakened, or when there is repugnance on the part of the patient, and those around him, to admit what argues a dangerous disease at hand. The influence of these causes is well known to every physician.

The use of Emetics in producing absorption of effused fluids, or of parts morbidly enlarged, seems sufficiently attested. But we have no reason to suppose that they have any effect on tubercles actually formed; and their employment therefore in phthisis, when we can obtain trial for them at all, is probably to be limited to the earliest stage of the disorder.

I need advert but slightly to the benefit derived from Emetics in asthma; though here again it must be admitted that there is an insufficient use of the remedy, seeing the great good gained in many such cases by unloading the stomach and liver; and the equal advantage, though less obvious in explanation, from its influence on the actions of the circulation and nervous system. A single Emetic may cut short a paroxysm for which opiates and antispasmodics have long been employed in vain.

The use of this remedy in the disorders of children is at present much less general than it ought to be. In very many cases Emetics would beneficially supersede that employment of purgatives, which often adds to the irritation it professes to remove. In the infantile fever, for example, which is a type of various

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disorders, an occasional dose of ipecacuanha, so as to excite vomiting (especially where there is much of the cough which attends this complaint, and large secretion from the mucous membrane), will be found more effectual than any other means. It is to be noted, further, that the action of vomiting is for the most part singularly easy in children;—more immediate, and generally less distressing, than that of purgative medicines.

In these remarks I have attended rather to the general effects of Emetics, than to the several qualities and manner of action of the medicines so termed; it being my object only to draw attention to the fitness of their larger and more defined use in ordinary practice. The question of preference among different emetic substances is indeed of less moment, from their action in emptying the stomach of its contents. In the majority of cases, that may be deemed best which fulfils its purpose without actual pain, and with greatest certainty and speed. If ipecacuanha were invariably of good quality, which unfortunately it is not, it might be sufficient in almost every instance. Antimonials, from sustaining nausea longer, and producing more distinct sedative effect on the nervous and vascular systems, may be preferred where excitement of these exists. The emetic of simple mustard ought never to be lost sight of as an immediate resource, and as producing its effect with less previous nausea or distress than any other.

In considering, however, the effects of Emetics, we must separate such as belong to the direct action of the medicine on the coats of the stomach, from those produced by the act of vomiting. The former may be more or less hurtful, as depending upon a peculiar irritation of the part; and here the action of emetic medicines must be assimilated to that of acrid or poisonous matters received into the stomach or generated there. But the mere effort of vomiting itself is much less injurious to this organ than might on first view appear likely. Without referring in proof to the "*vomitus luxuriæ causâ*" of the Roman dinner tables,[1] I may again mention sea-sickness, the sickness of preg-

[1] Celsus, lib. i, cap. 3. Many of my readers will recollect that remarkable letter of Cicero to Atticus, describing the dinner he gave to Cæsar at Puteoli, a short time before the death of the great Dictator. The *ambulatio in littore*, the *balneum*, the *unctio*, are all related in the order of their occurrence: followed by the emetic, with which Cæsar did honor to the banquet of his illustrious host, eating and drinking afterwards, as we are told, "ἁδεῶς et jucunde; opipare sane, et apparate."

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nancy, the frequent vomiting of infants, and the habit some individuals have of rejecting constantly in this way a portion of all food taken, as evidence that little mischief comparatively can be derived from this source.[1] And in the latter case of habitual or periodical vomiting, it may be remarked that the effort is usually attended with little nausea, though often with distension or other uneasiness preceding the act. These points must always be kept in view in judging of the effect of Emetics in practice, and of the frequency and particular methods in which they may best be employed.

I shall be gratified, because believing it to be a good result, if the preceding remarks should have any effect in restoring to its fit place in the treatment of disease, a remedy sanctioned to us by the oldest usages of medicine; and yet more by the safety, speed, and completeness of all its effects. I know none equally capable in so many cases of arresting disorder in its earliest stage, without shock or injury to any organ or function of the body;—or of abating the severity and duration of disease, where its sudden suspension is unattainable.

[1] I have known a patient, a young lady of delicate and irritable habit, who, during fourteen months, rejected invariably some portion of every meal;—the quantity rejected, and the interval before vomiting occurred, depending much on the quality of the food. In this case (in which the malady depended not on organic disease of the stomach or pylorus, but seemingly on irritation translated from another part), the patient suffered chiefly from an uneasy distension preceding vomiting, little from nausea or the vomiting itself; and she even gained considerably in flesh while the disorder was going on.

In another instance I have known the habit of vomiting continue for many years, after every meal of which animal food was the principal part; without any apparent injury to the constitution, or increase of mischief to the stomach itself.

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CHAPTER XXV.

ON THE USES OF DILUENTS.

THOUGH there may seem no sufficient reason for considering these as a separate class of remedies, yet I doubt whether the principle of treatment, implied in the name, is adequately regarded in modern practice. On the Continent, indeed, the use of diluents is much more extensive than in England; and, under the form of mineral waters especially, makes up in some countries a considerable part of the general treatment of disease, especially of all such as assume the chronic form. The practice of resorting annually to some *watering place*, though much governed by mere fashion, and not free from the worse influences of quackery and fraud, yet is undoubtedly beneficial in a vast number of cases;—partly in effect of the mineral ingredients in these springs;—partly (perhaps principally) from change of air, scene, and society, and the more salutary manner of life associated with the remedy.[1]

In very many cases, however, part of the good thus attained is certainly due to the water itself, irrespectively of the mineral matters it contains. And the consideration comes before us in an explicit form, to what extent, and with what effects, this great diluent, the only one which really concerns the animal economy, may be introduced into the system as a remedy? Looking at the definite proportion which in healthy state exists in all parts of the body between the aqueous, saline, and animal ingredients —at the various organs destined directly or indirectly to regulate this proportion—and at the morbid results occurring whenever it is materially altered, either by deficiency or excess—we must admit the question as one very important in the animal economy,

[1] We have proof of similar use and fashion as to mineral waters in the Roman practice. Pliny speaks of the "*Medici qui deverticulis aquarum fallunt ægrotos.*"

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and having various relation to the causes and treatment of disease.

Keeping in mind then this reference to the use of water as an internal remedy, diluents may be viewed under three conditions of probable usefulness. *First*, the mere mechanical effect of quantity of liquid in diluting and carrying off matters, excrementitious or noxious, from the alimentary canal;—*secondly*, their influence in modifying certain morbid conditions of the blood;—and *thirdly*, their effect upon various functions of secretion and excretion, and especially upon those of the kidneys and skin. Other more specific effects there may be; but these are presumably the most important; and each of them is capable of contributing to valuable ends in practice.

The first is an obvious benefit in many cases, and not to be disdained from any notion of its vulgar simplicity. It is certain that there are many states of the alimentary canal, in which the free use of water, at stated times, produces good which cannot be attained by other or stronger remedies. I have often known the action of the bowels to be maintained with regularity for a long period, simply by one or two tumblers of water, warm or cold, on an empty stomach; and this in cases where medicine had almost lost its effect, or become a source only of distressing irritation. The advantage of such treatment is still more strongly attested, where the secretions taking place into the intestines, or the products formed there during digestion, become vitiated in kind. Here dilution lessens that irritation to the membranes which we cannot so readily obviate by other means; and aids in removing the cause from the body with less distress and disorder than any other remedy. In some instances, where often and largely used, its effect goes yet farther, in actually altering the state of the secreting surfaces, by direct application to them. But it is difficult in the greater number of cases, to distinguish this result from the effect taking place through the circuit of the blood.

I mention these circumstances upon experience, having often obtained much good from resorting to them in practice, when stronger medicines and ordinary methods had proved of little avail. Dilution thus used, for example, so as to act on the contents of the bowels, is beneficial in many dyspeptic cases, where it is especially an object to avoid needless irritation to the sys-

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tem. Half a pint or more of water taken when fasting, at the temperature most agreeable to the patient, will often be found to give singular relief to his morbid sensations; and, where such is the effect, may even become a valuable aid to the other treatment pursued. Or, in cases where there is habitual excess of acid in the lower bowels (a source of frequent distress, though not so easily recognized as acid on the stomach), the solution of half a drachm of the Bicarbonate of soda in the quantity of water taken will add greatly to the good gained. It is often more beneficial in this way than given in smaller proportion of liquid; —a point illustrating the action of mineral waters, which clearly influence by quantity and dilution the medicinal effect of their contents, while having at the same time the mechanical operation due to water alone.

In reference to the foregoing uses of diluents, it is to be kept in mind that the lining of the alimentary canal is to all intents a surface, as well as the skin;—pretty nearly equal in extent;—exercising some similar functions, with others more appropriate to itself;—and capable in many respects of being acted upon in similar manner. Medical men themselves, and still more those with whom they have to deal, are prone to attach undue importance to the mere fact of a substance being taken into the stomach; as if this were equivalent to its being received into the system. Though the apparatus for absorption and secretion is different for the two surfaces, yet may we regard substances in the alimentary canal, prior to absorption, as being exterior to the system, nearly in the same sense as if applied to the skin. As respects the subject before us, it is in many cases both expedient and correct to regard diluents as acting on this internal surface, analogous to liquids on the outer. And I would apply this remark, not only to the mechanical effects of the remedy, but also to their use as the medium for conveying cold to internal parts; a point of practice which either the simplicity of the means, or the false alarms besetting it, have hitherto prevented from being duly regarded.

The abstraction of heat from an inflamed or irritable membrane within, is often indeed quite as salutary as the cold directly applied to a hot and dry skin without. The extent of use is from obvious causes much more limited; but I have seen enough of the benefit from cold liquids freely given in the acute stage

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of gastric disorders, inflammatory and febrile, with express reference to this point of temperature, to justify the recommendation of more frequent recourse to it in practice. This is a point where the feelings and desire of the patient may fairly be taken in guidance; and we can rarely go wrong in following them. The test, in fact, is simple and immediate; depending on sensations which cannot readily be mistaken, and the changes in which not merely suggest the use of the remedy, but indicate the manner and fit extent of its employment.

The second condition under which diluents may be viewed, as altering certain morbid states of the blood, is one of more difficulty; and connected with questions in physiology and pathology still under active research. Independently of those recent observations of physiologists which show the natural difference of proportion between the fibrine and serum of the blood in the two sexes, and under different ages and temperaments, we know as matter of fact that there are such morbid states, in which the proportion of water to the solid animal contents of the blood is below the healthy standard. Slight variations of this nature, as well as those of opposite kind, may be presumed to be perpetually occurring; delicate and beautiful though the adjustments of function are, through which the balance is maintained, and all inordinate deviations arrested in their progress.

But passing over lesser instances, the singular facts observed as to the blood in the Asiatic Cholera show the extent to which such changes may take place under the influence of disease, and give wide scope to the existence of gradations between this and the state of perfect health.[1] The importance of these changes of proportion, however produced, is best inferred from the experiments which prove how essential are the fibrine and coloring particles, not merely to the right constitution of the blood, but to all the phenomena of life—how many functions concur and are needful to their elaboration—and how speedily distress or death ensue in many cases where they are exposed to the agency of foreign substances introduced into the circulation.

[1] Andral, in his "Anatomie Pathologique" (vol. i), has given a summary of most of the facts in this interesting part of pathology; and Lecanu, in his "Etudes Chimiques sur le Sang," makes this specification more distinct by the results of very exact analysis. It is part of our knowledge likely to be extended hereafter.

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The question with which we have now concern is, how far the actual use of diluents may be capable of repairing such altered proportion of the serous part of the blood, whether the result of chronic causes, or of more acute malady. The tendency of modern inquiry regarding absorption has been to show that it takes place much more rapidly than was supposed; and through textures, and by means, which were once believed to have no concern in this function. The conception that the processes of absorption, as well as those of secretion, must be carried on through the open mouths of the vessels, and that communication of fluids can only take place by means of tubular structure, has given way to the distinct experimental proof of imbibition and exudation, through the coats of vessels or other parts of animal texture. In the phenomena of Endosmosis, more particularly, we find curious and satisfactory explanation of facts as to the transmission of substances by the animal fluids, which formerly perplexed all our views of the economy of structure and function.[1]

These points, however, are only important to us as proofs of the facility and speed with which liquids taken into the stomach, especially when fasting, may be absorbed into the circulation; and it still remains a question, how far the blood can appropriate to itself, in alteration of its quality, fluids thus received. There can be little doubt that it does so, in those cases where the proportion of its watery parts is from any cause materially lessened.[2] In that intense thirst, passing into febrile state, which follow the long privation of liquids, and appears to be the most vehement of all appetites, there is every reason to suppose that these feelings are the result of changes in the blood itself; depending on the altered proportion of its fluid and solid parts, and affecting the vascular system to its most minute extremities. Perhaps in

[1] These discoveries of Parrot and Dutrochet, continued and extended under different modifications by Graham and Matteucci, while remarkable as the exposition of a new physical law, have great interest in their application to physiology; and show especially how much may be added to our knowledge of the vital functions by every such step in physical science. The views as to respiration, suggested by Dr. Steven's valuable observations on the affinity of carbonic acid and oxygen, afford another instance of similar kind, having various relations indeed to the former.

[2] The experiments of Orfila and other physiologists have shown the connection between thirst and the proportion of water present in the blood, by the relief given to this sensation from the injection of different fluids into the veins.

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all instances the tendency to absorption bears some ratio to the state of the blood in this respect, unless there be counteraction from causes of disease in active progress at the time. But it is equally probable, and for precisely the same reasons, that little alteration is made in its state by any excess of liquids beyond this natural demand. The processes for separating water become more active in the same ratio as the increased imbibition; and even admitting, what is now rendered very doubtful, that the whole or greater part passes through the blood, there is every reason to believe that the extrication thence, through different channels, is so speedy as never to allow any material change in the proportion of its several parts. There is manifestly a constant and mutual relation between the blood and the kidneys, the organ most especially destined to this function of balance. Whether change of condition begins in the former or the latter, equally is it followed by reciprocal change in the other, tending always, it may be presumed, to adjust or restore those proportions in the blood which belong to the natural state.[1]

We cannot indeed prove, where there is habitual excess in the quantity of fluid taken, that there may be not some effect of this kind; producing thereby a sort of chronic disorder of the blood, and affecting more or less every part of the system. A difference existing from natural temperament may be induced by habit also; but probably to no greater extent, unless the change be aided by the operation of actual disease. And, in the latter case, the altered proportion presumably depends rather on the insufficient power of supplying the fibrine and albuminous part of the blood, than on any change tending directly to increase the ratio of water contained in the circulation.

Applying these considerations to the medicinal use of diluents, it may be inferred that we have little justification for giving them, with direct intent to alter the qualities or proportions of the blood, independently of actual experience. Our knowledge on these points is still too limited to admit of any practice founded on theory alone. But, on the other hand, we have reason from the same considerations to believe that liquids may freely and

[1] Many extraordinary cases are recorded of the enormous quantity of water habitually taken, and urine voided, even without actual disease. In most of these it is difficult to determine where the excess of action begins, or by what produced.

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without fear be given, wherever there is demand for them from the sensations of the patient. There is, in truth, no motive or rule of treatment upon which better to depend than these natural feelings; and, even when partially vitiated in their nature, we have the assurance that it is a case in which the system is well provided with means for remedying any excess. The conclusion, fairly attained, is one of great practical value. And it ought sedulously to be kept in view, in a part of practice, where prejudice or false maxims are ever at hand to check and contravene the sounder judgment of the physician.

Viewing the subject, indeed, in relation to actual disease, when the appetites are in part removed from the dominion of habit, there are few rules in practice which may be admitted with such slight limitation as the one just given. Even in Diabetes, I have never found any continued good from opposing the inordinate thirst of the patient; nor aggravation of what are the essential symptoms of the disorder by assenting in full to this desire. The restraint as to liquids, sought for to slake the thirst in Cholera, is still less justified by any reasonable principle or experience in practice. Without admitting, what some have affirmed, that cold water is an actual remedy here, we may at least presume that it is beneficial, if finding access to the circulation, in modifying that state of blood which is so characteristic of the disease. The eager demand for liquid is in some sort an evidence of this; nor is it disproved by the occasional rejection from the stomach of what is so taken. The thirst occurring as an ordinary symptom of all fever, is for the most part to be viewed as authority for its gratification. In the greater number of cases, more harm will arise from opposing it, than from any excess in the use of liquids to which it can lead.

The remaining point of view under which we have to consider simple diluents, as remedies, is in their effect upon various functions of secretion and excretion. This subject is closely connected with the preceding, inasmuch as the effects must take place chiefly, if not altogether, through the circulation; and some change be produced in the blood itself by every alteration occurring in these functions. The kidneys and the skin are the organs most concerned here; though it is probable that the lungs also have part in the effect, and that no secreting organ is wholly independent of it. Any large and rapid addition of water to the

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mass of blood brings into activity that part of the vascular system, whether glandular or otherwise, through which separation of the superfluous fluid may take place. The pressure of volume upon the extreme vessels or upon the membranes through which exudation takes place, must have influence on the functions of these parts:—and the same might be inferred, if we regard as vital phenomena exclusively, the actions by which superfluous fluid is thus separated from the system. Under either supposition the secreting vessels or exuding membranes certainly undergo alteration for a time; and this of nature to affect the qualities of the blood, by altering the amount or kind of matter which such secretions carry away. In the kidneys, for instance, the proportion of the lithates or other saline ingredients, removed through these organs, may be augmented by a large and rapid passage of water through them; and the functions of the skin, as an excreting organ, may equally be rendered more active by the same means. It is probably in this indirect way that diluents have greatest influence upon the blood; and here we find best explanation of their utility in certain cachectic cases, in which there is general disorder or deficiency of the secretions; and in rheumatic fever where the condition of the blood is evidently concerned as the main cause of disease.[1]

It seems certain, further, in the medicinal use of diluents, that something is gained by mechanical effect on the secreting tissues. These may become in any part less permeable than in the healthy state; a condition apparently capable of being removed by the more copious passage of fluid through them. The remark applies to many states of the skin, both in fevers and other disorders, where simple diluents, freely given, form the safest and most effectual sudorifics we can employ. And the same remark applies to various cutaneous disorders, where the use of this remedy, even in its simplest form of pure water, might be very beneficially extended, to the exclusion of many diaphoretics and alteratives of much more doubtful character and effect.

This is, according to my experience, a point singularly merit-

[1] Those familiar with the writings of Sydenham, may recollect the report he makes of his large and beneficial employment of *whey*, in certain severe cases of rheumatic fever, to the exclusion of all other nutriment. In one case he thus used the whey from eight pints of milk daily, for four successive days. Facts of this kind are not to be disregarded for their seeming simplicity; especially when coming to us with the name of Sydenham annexed to them.

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ing attention in practice; and the simplicity and safety of the trial warrant every recommendation of it. In exanthematous fevers, though the treatment by such means is more ambiguous, yet I believe that a much freer employment of diluents than is commonly adopted might be beneficial; certainly to the fullest extent in which they are called for by the feelings of the patient. I can affirm, after much observation, that I have never seen harm result from this treatment—in the great majority of instances unequivocal good.

On the same principle, there is much reason for their use even in certain cases of dropsical effusion, where the main object is to restore a due action of the kidneys. Popular feeling, and indeed medical opinion, inclines to the opposite treatment; and with some reason, as respects any constant habit of drinking largely in such cases. But I have experience of the good often obtained by the sudden and copious use of diluents in recovering these organs from a dormant state—an object which, I may remark, is generally best effected after the bowels have been fully relieved.

The latter fact has some importance in regard to the action of almost all diuretics. They have little effect as such, while there is need of alvine evacuation; and in ascites and anasarcous swellings, particularly, it is commonly desirable to preface all other treatment in this way. The ignorance or neglect of these precautions in practice often produces a hurtful activity where nothing can be gained, and a delay no less injurious in the actual relief of disease.

As I have been treating of this remedy only in its simplest form, I do not advert to the use of the different mineral waters further than to state that they confirm these general views; separating, as far as can be done, their effect as diluents from that of the ingredients they contain. I have already alluded to their more copious employment in Continental practice; giving room for observation, which is wanting under our more limited use. I have often seen five or six pints taken daily for some weeks together (a great part of it in the morning while fasting), with singular benefit in many cases to the general health, and most obviously to the state of the secretions. The functions of the kidneys and skin were maintained in great activity during the whole of this period; but without assuming any disordered

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character; and passing readily again into their natural state when the cause of increased action was removed. These courses, however, were always conjoined with ample exercise and regular habits of life; doubtless influencing much the action of the waters, and aiding their salutary effect. In cases where such aids are omitted, or only partially employed, a much smaller quantity generally disturbs the stomach, and the treatment altogether is of little avail for the amendment of health.

Without reference, however, to these extreme cases, it must be repeated, that the use of water, simply as a diluent, scarcely receives attention and discrimination enough in our English practice. This is a point wholly distinct from the question regarding the fit proportion of liquids *as a part of diet*. The process of digestion suffers more or less from any excess in quantity of these; and though the natural appetite may be unduly controlled, yet a rule is generally required, in dyspeptic cases especially, to obviate such excess, even where the simplest and most innocuous liquids alone are concerned. For in these cases a morbid craving for them is often created; partly by the vitiated sensations of the patient; partly from the actual state of the membrane lining the palate, œsophagus, and stomach, and from the disordered secretions and products of digestion acting on this surface.

It is obviously another question, how far and in what way diluents may be employed, expressly as such, for medicinal purposes;—including under this question what relates to thirst as a symptom of disease; to the use of liquids in disordered states of the alimentary canal; and to their employment in cases of general cachexia and vitiated secretion. These are the points to which the preceding remarks apply; less, however, as furnishing explicit rules, than as suggesting the views upon which this part of treatment may best be founded, and rendered more generally useful.

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CHAPTER XXVI.

ON SUDORIFIC MEDICINES.

It must have occurred to the observation of most practitioners, that, of the direct evacuant remedies, Sudorifics are amongst those which can least be depended upon; and this even in cases where natural perspiration is one of the symptoms statedly present. It will, I think, be further admitted that perspiration produced by artificial means rarely corresponds, in the degree of benefit obtained, with that occurring from natural causes;—and, further, that it is a class of remedies where discrimination of effect is singularly vague as regards the several medicines so denominated. These indeed are points which different observers will differently estimate; yet I believe the facts will be so far allowed, as to give some interest to a consideration of their cause.

Does not this uncertainty chiefly depend on mistaken views of perspiration, as a part and symptom of disease? In general opinion, it seems to be regarded as an active cause of change, and usually of amendment, in the existing disorder; and practice is founded for the most part upon this impression. May it not more justly be considered as effect and proof of such change taking place; frequently beneficial in kind, but by no means invariably so? The distinction is obviously important; inasmuch as it involves the question, how far, and by what means, this action is to be sought for as a direct object in the treatment of disease.

In their employment of sudorific remedies, physicians at all times have manifestly dwelt much on the fact, that many disordered actions, and especially those of the simple febrile paroxysm, and certain of the phlegmasiæ, are suspended in immediate sequel to natural sweating. Proofs as to this mode of inference might be largely drawn from medical writers, were it necessary;

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nor is the reasoning devoid of plausibility. Setting aside all questions of peccant humors, an obvious and general evacuation from the body, followed by mitigation of disorder, is a fact in pathology which could never escape notice, and might readily suggest the idea of like advantage from perspiration artificially obtained. Nevertheless, there is a great cause to doubt the justness of the inference, and the propriety of much of the practice which has been founded upon it.

In this inquiry, it is needful first to consider, how far the natural action, which we seek in other ways to imitate, is truly remedial in kind. It would advance us very little here to enter into the various theories as to the functions of the capillaries and exhalant vessels. The probable statement seems to be, that what is termed perspiration in the widest sense involves two separate processes;—one a mere physical act of simple evaporation of moisture from the surface,—the other an act of specific secretion by vital power, and affected therefore expressly by the various states of the circulation and nervous system. The latter is that with which we are here concerned; and, looking to the character of this function, it must be admitted as not impossible that what are called critical sweats may be really the cause of the relief which ensues upon them. But this admission by no means proves the fact to be so; and there are various presumptions, more or less strong, which may be brought against it.

First, the occurrence, often observed, of similar and equal relief under the same course of previous symptoms, without intervening perspiration. This will be familiar to all who are accustomed to note the phenomena of fever. Here remissions or intermissions take place; always, no doubt, with an altered state of the vessels of the skin, but often with little or no actual perspiration succeeding to the hot stage. Even in the simple paroxysm of ague, which affords the best case for the inference now in question, this absence of a distinct sweating stage is occasionally observed: in other forms of fever, idiopathic and symptomatic, much more frequently. It may be difficult to say, why this resolution of fever or inflammation should occur in some cases with copious perspiration, in others with little or none. But, the fact being so, it is safer and probably more correct to regard the sweating stage or critical sweat, when it happens, as

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one in the series of changes constituting fever, rather than as the cause of the relief to symptoms that ensues.

The second point I would advert to is, the frequent occurrence of natural perspiration in morbid states of the body, without any such relief, and sometimes with aggravation of disorder. Proofs of this may be drawn from common experience in continued fevers, pneumonia, acute rheumatism, and various other diseases attended with fever, where profuse or long-continued sweating often occurs, with doubtful benefit or even manifest disadvantage to the patient.[1] The perspirations in hectic fever, though marking the several periods of remission, yet augment the distress of the patient, and increase his weakness. In the fever attending the epidemic influenzas, the more general state of the skin is that of a clammy perspiration, manifestly not producing any remission of the symptoms. Even in the exanthematous fevers, where it might be inferred from the eruptive symptoms that diaphoresis would be more uniformly beneficial, and a legitimate object of treatment, cases constantly happen where natural sweating is attended with no obvious good; and where the attempt to force it out by medicines or other means is distinctly injurious to the patient.

Many other instances will occur, more or less directly in illustration of the same point. In those complaints usually termed bilious, and where there is accumulation, or disordered change of secretion, in the digestive organs, perspirations break out often and copiously, without any relief to the system. The same happens in various other disorders of the alimentary canal; an effect of the intimate sympathy between this great internal membrane and the surface of the body. A tendency to perspiration is the consequence of purging in any excess; as indeed of all causes which, even without fever, tend to debilitate the body. These instances, though less determinate than those of simple and symptomatic fever, yet illustrate the same general view. If perspiration be regarded as an index of the change of symptoms, rather than as the cause of this change, it may be expected to

[1] The remark of Hippocrates is warranted by fact, ἱδρως πολυς ἁμα πυρετοισιν ὀξεσι γιγνομενος φλαυρον. And elsewhere, κἀν πυρετοισιν ἱδρως ἐπιγενηται, μη ἐκλειποντος του πυρετου, κακον. These are among the numerous prognostics which he derives from perspiration, as a symptom in the course of disease, and an exponent of changes taking place in the body.

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occur in many cases, where it does not indicate any favorable alteration, and where in fact none such takes place.

Looking, indeed, to the various circumstances under which this symptom appears, we have reason to infer that the state of the exhalant vessels producing it is not a single and uniform one; but that at least two, possibly more, conditions are necessary to explain the different manner in which perspirable matter is thrown off. I doubt whether sufficient inquiry has been directed to this point. It seems probable that one of these states may be simple relaxation of the vessels; the reverse of that which exists during the hot stage of fever, and indicating therefore a more entire removal of the febrile state:—another, a forced and active state of the same vessels, by which fluid is thrown out, but with little diminution of heat or febrile symptoms. Other causes of variation might be conjectured, to explain these differences of effect, as well as the phenomena of partial sweats, indicating some unequal pressure on the vessels in different parts of the body.[1] Without attaching much weight to these explanations, it is certain that perspiration differs as respects the state of the vessels, and also as to the nature of the matter perspired,—the latter variation probably depending on the former in most cases; though there may be others where the reverse is true, and where the condition of the blood in the extreme vessels may determine the state of the orifices through which exhalation takes place. On this subject I have made some observations; but they need to be extended and confirmed before I can state them in distinct shape.

Recurring, however, to the question of Sudorifics as a remedy, it may be alleged that something is due both to the quantity of perspirable matter thrown off by the skin, and to the quality of this, as ridding the blood of superfluous or noxious ingredients. It must be admitted as probable that both these causes have influence. But the little proportion in many cases between the amount of perspiration and the degree of relief attending the remission of fever, and the frequency of copious perspiration without any relief at all, proves that this at most is only partial

[1] The ancient physicians attached more importance than we do to the "Sudores non per totum corpus æquales," as a bad symptom in disease. They are connected doubtless, with the same general causes which produce local determinations of blood, of vascular action, in different parts of the body.

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and accessory. As respects the latter circumstances, viz., the nature of the matter thrown off, it offers undoubtedly a question of greater difficulty. We have not yet sufficient knowledge of the varieties of this, especially under morbid conditions of the body, to be entitled to deny the influence of such changes; and the researches which show that a function analogous to that of respiration is exercised by the skin, and varied by disease, make it probable that, in perspiration, the excretion of carbon through this channel may be considerably increased in amount.[1] Still it is very doubtful whether these presumed changes of quality in the matter perspired, can have much effect on the course and character of disease; seeing that perspiration is one of the least constant symptoms, even where usually present; and that the relation is very uncertain between its amount and the relief that ensues.

The presumed influence of perspiration in reducing animal heat admits of the same general remarks. Even admitting that there is no doubt as to priority in the order of occurrence, the frequent continuance of morbid heat under excessive perspiration shows that there is no certain ratio between the two symptoms, and lessens the value of this relation to the purposes of practice.

Looking cursorily to medical history on the subject before us, it would appear that the use of internal sudorific medicines was comparatively infrequent among the Greek and Roman physicians. Collecting the passages in Hippocrates which relate to this point, I doubt not that Dr. Friend was justified in believing, that by this great physician sweating was regarded much more as a prognostic sign than an instrument of cure.[2] The practice, even if originating earlier, gained ground chiefly in the middle ages; was extended by the larger introduction of chemical medicines into the Pharmacopœias; and naturally allied itself with the humoral pathology, and the notion of expelling peccant

[1] The experiments of Dr. Edwards on the Batrachians with soft skins show to what extent this function, resembling respiration, may take place through the cutaneous vessels; and confirm the observations which had before been made on the human body. From the researches of the same eminent physiologist, as well as the anatomical observations of Breschet on the skin, we are entitled to infer a distinction in quality of ingredients between perspiration by secretion, and the simple moisture which is evaporated from the surface of the skin.

[2] Etenim sudor perpetuò apud Hippocratem non ut curandi instrumentum, sed tantum ut præsagii nota præponitur.—*Comment. Novem de Febribus*, Londin. 1717.

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matters from the body. It is well known how far the custom was carried, at a period not distant from our own, of forcing perspiration by external heat, confined air, and stimulating sudorifics; and this more especially in fevers and exanthematous disorders. No more beneficial change has occurred in modern practice than the abandonment of these methods; nor any more conspicuous in its effects on the rate of mortality in disease. Yet we have cause to ask whether there is not yet too much influence from this source; supported as it is by popular prejudices, and by maxims transmitted from one generation to another. The excitement of perspiration is still often made a direct object of treatment in cases where, even when not injurious, it has little value except as an exponent of the state of other symptoms.

Nor indeed can we draw this inference equally from perspiration artificially obtained, as from that of natural occurrence. In many diseases, perhaps more than our medical philosophy has hitherto dreamt of, there is much mischief done by forcibly disturbing and altering the train of symptoms which forms the wonted course of the disease. No point is more difficult in the theory of medicine, none which puts to closer test the practical judgment of the physician, than the knowledge where boldly to break into, or cut short, the cause of a malady,—where to confine the treatment to such means as may simply mitigate the symptoms, obviate any irregularity in their course, or prevent injury to particular organs. The question is one which, in connection with the doctrine of a *vis medicatrix naturæ*, has had influence, more or less direct, upon medicine and medical schools in every age. In some diseases there can be no reasonable doubt which of the respective plans should be pursued. But even in these more marked cases there is need of greater discrimination than is usually employed; and to the young practitioner, more especially, it is often matter of much difficulty, not only to form his judgment, but to carry this judgment into action. A firmness of mind is requisite, which may show itself in boldness and vigor, when these are needed; and no less in forbearance (whatever the solicitations to the contrary), when a disease is pursuing its course mildly, and without any unwonted symptoms.

This forbearance, often the most difficult virtue, may fairly be inculcated as to the use of sudorific means, at those periods of

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diseases of definite course where sweating does not generally, or naturally, occur. The attempt to force it at such times, even if successful, is a very dubious benefit; and under many methods of treatment for this purpose becomes a positive evil. The effect in question, however, is by no means so easily obtained as we commonly suppose. The perspiration attributed to medicines is often, as already remarked, a symptom in the natural progress of the malady, or depending on other causes in the animal economy. We have, for instance, every reason from direct observation to conclude that the state of sleep, as such, tends to increase perspiration, separately from all influence of other agents. I doubt not that the diaphoretic medicine often obtains for itself the credit of an effect due chiefly to this cause. Even where more obviously the result of internal remedies, it would seem that these are effectual in the greater number of cases, rather by abating fever, and changing the course of actions in the system, than by direct influence on the exhalant vessels.

The employment of sudorifics may indeed be regarded, on the whole, as less determined by method or reasonable experience, than that of other evacuant remedies; and every candid physician will admit his frequent disappointment in this part of the treatment of disease. The quantity of what is perspired cannot be determined with any exactness: its quality is very seldom known sufficiently to become an index of practice; and there is ever much difficulty in deciding whether the effect taking place is due to means employed, or to the changes, natural or morbid, of the body itself.

These doubtful views on which their general use is founded, will best explain the uncertain effects of the particular medicines brought under this class. The Antimonial preparations, though so largely employed from their presumed action on the skin, may nevertheless be noted in proof of this. There is reason to suppose that their beneficial effect in fevers and inflammations is mainly owing to the influence they have in diminishing the force of circulation; and thereby lessening the febrile action, which prevents secretion from the surfaces as well as from the glandular textures of the body. And their employment is generally more successful in proportion as this principle is kept in view, and the act of perspiration regarded as an effect of secondary kind.

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The reputation of James's Powder as a diaphoretic may have depended in part on this sedative action upon the circulation; but still more, as I believe, upon the sweating regimen employed together with it. The comparative neglect of this regimen, as well as the smaller and less frequent doses of the medicine now used, will explain its lesser efficacy in modern practice, of which we often hear complaint. There is, besides, some reason to presume that its value was always rated too high.

The latter remark will apply further to the use of antimonials in cutaneous disorders, where a direct effect on the skin is sought for. A fair estimate of the evidence here will lead to reasonable doubt whether this practice is ever of much avail. The proofs indeed are usually so far obscured by the conjunction of other means that little can be securely gathered from them.

With respect to the stimulating sudorifics, as they have been termed, it may fairly be presumed that most of them act as such only by exciting a certain degree of febrile action, followed by relaxation of the exhalants;—a doubtful benefit in any case, and often a certain source of mischief. Sydenham strongly reprehends this practice as it existed in his time; and other physicians have, at succeeding periods, repeated his censure. Yet even now it cannot be said to be wholly discarded.

Of the various internal means for obtaining diaphoresis, I believe that Opium, in one or other of its forms, is the most uniformly certain and beneficial. Its action, in conformity with the views just stated, may be considered to depend on the power of allaying inordinate circulation, or other excitement of the system. This opinion is justified, as well by the other effects of Opium, as by the similar influence of various narcotics on the state of the skin.

I allude more especially to Opium as a diaphoretic, because I believe that in our modern practice we too much neglect the various and beneficial resources afforded by this medicine. Its influence upon the vascular system, in particular, is of singular importance in the treatment of disease. Whatever be thought of Lind's recommendation of Opium in the hot stage of ague, it is certain that in various stages of fever and inflammatory disorder, where preceded by sufficient evacuations, no medicine can be more safely used as a sudorific, or with greater collateral advantages. This is strongly urged by Dr. Friend, in the third of his

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Commentaries on Hippocrates; to which I would further refer, as containing some excellent observations on the sudorific plan of treatment in fevers;—one of the earliest efforts to change the opinions before held on this part of practice.

I have spoken elsewhere of the use of simple diluents as a means of exciting perspiration, too much neglected in modern English practice. It is connected with that close relation of the skin and internal surfaces which it is so important to keep in mind in every part of pathology and practice. The influence of bloodletting in producing perspiration doubtless arises from its diminishing what may be called, in default of a better term, the tension of the general circulation; and thereby relaxing the vessels of the skin and other surfaces. Here then, also, while admitting that some benefit may be derived from the perspirable matter thrown off, we must regard the act of perspiration principally as an index of the favorable change taking place in the vascular system at large.

Limiting these remarks, however, chiefly to sudorifics of internal use, I say nothing of the various external means employed to the same end; even though they might illustrate in many points the views already stated; and particularly as regards the relative effect of cold and hot applications to the skin, in promoting a free action of the exhalants, under different morbid conditions of the body.[1] Nor do I propose here to apply these views to particular cases of disease. My object is merely to offer suggestions which may tend to render our notions on the subject more simple and precise; and I venture to lay down the following positions as founded on sufficient evidence.

First, that it is more reasonable, as well as beneficial in practice, to have regard to the changes in the circulation producing diaphoresis, than to the action of sweating itself. And, *secondly*, that the amount of perspiration is rarely a just measure of the good obtained; and that to make this a primary object is likely to give a wrong and injurious bias to the treatment of disease.

It may be alleged that these are needless refinements of prin-

[1] The practice of Hydropathy has afforded some remarkable facts in connection with this part of physiology and therapeutics. As I have said elsewhere, it is impossible not to foresee in this practice, after its excesses have been curtailed, and its useful applications better determined, much that may furnish important aid in the treatment of disease.

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ciple, not required by the present state of medicine among us; and I willingly admit that there has been in this instance a greater change in actual practice than in the general views upon which it is professedly founded. It was impossible, with increasing exactness of observation, to escape the conviction that in febrile and inflammatory disorders the direct cooling methods of treatment are the safest and most beneficial; and these accordingly have been gaining ground to the exclusion of such as were previously in common use. Still there remains something to be done to render this change more general and complete. And yet further, it is of importance to give stability to all such practical results, by fixing them upon principles which cannot be changed with the fashions that affect particular medicines or methods of treatment.

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CHAPTER XXVII.

ON THE USE OF OPIATES.

The manner of employing Opium in modern practice might, until very lately, have been cited among the many examples of perverse changes of fashion as to particular remedies and methods of treatment. All that tended to increase the credit and use of purgative medicines lessened for a time, in the same ratio, the reputation and employment of this remedy. A contradiction was presumed, greater than actually exists, between the two modes of treatment. The fear of confining the bowels and checking the secretions, constantly present to the mind of the practitioner, readily imbued the patient with the same alarm. And thus was prevented the adequate use of a medicine, having the power of mitigating pain, of relieving spasm, of procuring sleep, of producing perspiration, and occasionally even of aiding the natural action of the bowels, by obviating the disordered actions which interfere with this function.

I speak of this as *having been*, because it is certain that opiates are again more largely employed, since the introduction of Morphia, as a common preparation, has furnished new methods of administering the remedy, and revived attention to the principles of its action. Yet even now it may be affirmed that there exists a distrust both as to the frequency and extent of its use, not warranted by facts, and injurious in various ways to our success in the treatment of disease. This is the more singular, seeing the boldness of our practice in other points; that we have in the sleep produced a sort of limit and safeguard to its effects; and that we possess remedies of easy application for all injurious symptoms that can arise. To the insufficiency, indeed, of the quantities given may be attributed in some part the comparative disregard into which the remedy fell during a certain

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period. Half a dose might disturb and distress the night which a full dose would have made one of perfect rest; or perplex the aspect of symptoms which a larger quantity would have alleviated or removed.

Yet medical experience does but follow common observation in recognizing the inestimable value of sleep in sickness; of the suspension of pain, and the check to all disordered actions, thereby obtained. For pain and sleeplessness, though strictly but symptoms of other ailment, may often in practice be viewed as disorders in themselves, the removal of which is essential to the success of our general treatment.[1] In a preceding chapter, I have treated especially of pain, as a symptom of disease; and of the means by which it may best be obviated, including Opium as the most important and valuable of these. But apart from the more acute suffering of disease, how often do we see a nervous restlessness come over the patient, the consequence of protracted illness or other causes—retarding cure, by preventing the due effect of remedies, and receiving no relief itself from the means employed for the original disorder. In such cases the physician must not submit himself to names or technicalities. The regular course of treatment must be suspended till the hinderance is removed; and even seeming contradictions to this course may safely be admitted for the attainment of the object. Here Opium is the most certain and powerful of the aids we possess; and its use is not to be measured timidly by tables of doses, but by fulfilment of the purpose for which it is given. A repetition of small quantities will often fail, which concentrated into a single dose, would safely effect all we require.

On this subject we must refer to a fact regarding Opium, singular in itself, and affording many curious inferences. I mean, the frequent absence of its ordinary effects in producing constipation, headache, and nervous symptoms, when given for the relief of acute pain or spasmodic actions in some part of the system. It would seem (however vague the expression) that the medicine, expending all its specific power in quieting these disorders of the nervous system, loses at the time every other in-

[1] A physician of large practice, whom I knew only in the latter days of his life, acted not unwisely (perhaps warily) in carrying about with him a box of small opium pills; with which he often gave relief, or made preparation for it, while yet in the house of his patient.

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fluence on the body. Even the sleep peculiar to opium appears in such instances to be wanting, or produced chiefly in effect of the release from suffering. Though certain analogies may be quoted in illustration of these facts, nothing like explanation of them is furnished by our present knowledge. Their reality, however, cannot be doubted as matter of medical experience; and the instances are further remarkable in the large doses which may frequently be given in such cases, with impunity as to all the ordinary effects of the medicine.[1]

These remarks apply further to certain cases, where, without any actual illness, there is a general irritability of constitution, carrying its morbid influence into all the functions of life. Here the use of Opium is for the most part admissible with less risk than in other temperaments; its injurious effects being in some degree merged in the relief given to this particular state of the nervous system.

The power of this remedy in checking various morbid actions in their earliest stage, even such as have no direct relation to the nervous system, is too much neglected in modern practice. In common catarrh, for instance, twenty or thirty drops of laudanum, or an equivalent dose of some other opiate, given with a warm diluent at bedtime, and followed in the morning by whatever laxative may be required, will often arrest altogether a complaint, which the later use of purgatives, antimonials, and saline medicines only tardily removes.

Nor is there, except in some cases to which I shall soon refer, any such risk as might seem likely from this sudden effect of Opium in checking the train of morbid actions, even in cases of more import than that just mentioned. The recommendation of its use, fairly attested by experience, in the treatment of the paroxysm of ague, may be cited as an instance in point. Where

[1] An instance is just now before me of a young lady of fourteen, who had never before taken opium in any form; but to whom from 140 to 160 drops of laudanum were given daily for many days in succession, to relieve the acute pain arising from inflammation of bone. Here, beyond the relief thus afforded, for which these large doses were essential, there occurred no obvious effect on any of the functions, bodily or mental, nor any need of counteracting remedies. Little assistance even was required for the bowels, though confinement of posture was added to the effect of the medicine. As the pain gradually abated, the peculiar influence of opium came to be more felt; and it was needful in the same ratio to lessen the amount employed.

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the check is given by the removal of irritation,—or of that which John Hunter has termed "alarm to the nervous system,"—or by the procuring of sleep, the effect is almost always safe and beneficial to the patient.[1]

In truth, the injurious effects of this substance as a medicinal agent are altogether overrated; a natural consequence of the mental and bodily evils resulting from its abuse when taken otherwise. I have seen the distresses produced by this excess, too frequently and too impressively, to allow me to offer a word in palliation of it. But there is nevertheless reason to say, respecting the general action of Opium on the animal economy, that no agent of equal and similar power produces its peculiar effects with less detriment to the constitution; nor is there any which admits of such extension of use, if gradually made, without fatal result. The excess in it is a hurtful intoxication, enfeebling both mind and body; but scarcely more so than the habitual excess in wine and other alcoholic compounds, with which we too largely and familiarly deal as a part of diet. Every sound and sufficient reason must lead us to limit its use strictly to medicinal purposes. But so limiting it, and in cases where it is rightly admissible as a mode of treatment, little apprehension need exist regarding the amount of dose; and inconvenience is at least as likely to arise from deficiency as from any ordinary excess.

In some cases there is doubtless an idiosyncrasy regarding Opium (as with respect to mercury, ipecacuanha, and other substances, whether in medicine or food), rendering it, even in minute quantity, a source of much injury to the nervous system. Instances of this kind occur, where the fact can no more be doubted than can the particular effect of tea and coffee upon some constitutions. Within the last three months I have seen two cases, in each of which there was distinct proof that Opium, in the ordinary dose, produced a great enfeeblement of voluntary

[1] It may seem strange, that while Lind strenuously recommends opium at the commencement of the hot stage of an intermittent, other authors have as strongly advised it at the beginning of the cold stage. We do not yet know enough of the mode of operation of the medicine through the nervous system, to be enabled to affirm that these opinions are incompatible, though it is probable that both cannot be just. It is a curious evidence how much is yet wanting to fixed principles of practice, even in the most familiar disorders, that the same contrariety exists as to the use of other powerful remedies in the cold and hot stage of agues.]

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power for some days;—in one of these instances affecting the right side only, and so powerfully as to give alarm of hemiplegia.

There seems also reason to believe, in conformity with the remark of some authors, that the effects of Opium are proportionally greater on young children, whether owing to the facility of absorption at this age, or to the greater susceptibility of the brain and nervous system;—a point, if well established, of obvious importance in practice. But all cases of idiosyncrasy regarding Opium require scrutiny; lest we bring under this head the mere errors of fancy or prejudice, or the effects of the medicine partially and injudiciously employed. I recollect very many instances, where I have had occasion to correct at a later time an opinion thus conceived, or assented to on insufficient proof.

It cannot be doubted that Opium is one of the most certain and beneficial means we possess of exciting general perspiration. This is probably an effect of its power of allaying inordinate action in the vascular system throughout the body; but there may be other causes also concerned, which are less obvious to us. I have spoken on this subject when treating especially of sudorific remedies; but I revert to it here, as a point scarcely enough regarded in general practice. In fevers, indeed, the use of Opium for this purpose is often prohibited by the other conditions of the malady; but there are many cases of slighter kind, where the perspiration and tranquillity, conjointly resulting from its use, suffice to arrest a disorder, which experience tells us might go much further were no such aid afforded.[1]

The employment of opiates in inflammatory diseases, and especially in such as affect the chest and head, offer many difficult points to the judgment of the practitioner. Here are morbid actions going on, of which we cannot safely for any length of time conceal from ourselves the amount. To stupefy the sensibility to pain, or to suspend any particular disorder of function, unless we can simultaneously lessen or remove the causes which create it, is often but to interpose a veil between our judgment and the impending danger. However beneficial are sleep and

[1] The usual doses of Dover's Powder, the form of opiate most frequently prescribed with this view, are too small for the object desired. Nor is there, according to my experience, sufficient reason for the preference given to this medicine over other opiates as a sudorific.

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ease, they may be purchased at too dear a rate in such cases; and we must seek them rather in sequel to the more active remedies by which the power of the disease is met and subdued. Frequently, indeed, no art can procure them before.

Yet it is certain that there is a character of some inflammatory diseases, and a time probably in all, where this remedy may safely and beneficially be admitted. In certain forms of peritoneal inflammation, whether puerperal or otherwise, attended with extreme pain and restlessness, and passing very soon into the typhoid state, relief to the condition which thus rapidly exhausts the vital powers is at all events to be sought for; and no appearance of distended bowels, or disordered secretions, can come in contradiction to this rule of greatest safety. Happily, the two modes of practice are not so incompatible as the more general use of the purgative treatment would seem to imply. The combination of calomel with full doses of opium is admissible and beneficial in most of these cases; and, where more express laxatives are required, they are usually best employed when the urgent irritation is removed. It is, for the most part, very difficult to render them effective before.[1]

Again, in cases of inflammation less special than the preceding, there is a period in each, succeeding to the early and more active treatment, when opiates have their appropriate value; not merely in giving rest, but also, as is probable, in lessening what remains of excess in vascular action. A right judgment as to the symptoms which indicate their use at such times, or even at intervals between the active remedies, is one of the best proofs of the ability of the practitioner; for no written rules, however plausible in show, can really minister certainty to this part of practice.

The use of Opium needs discretion in another class of cases, which have not, I think, been sufficiently adverted to in this light. I mean those instances, where the heart and organs of respiration are in a state so critical, as to enfeebled power, that the condition induced by opiates may suffice to turn the balance against them. This may happen either directly, by its influence on the nerves of these organs; or indirectly, by preventing the

[1] In the treatise on Puerperal Fever by Dr. Ferguson, published some years ago, I find a strong attestation of the success with which he has employed opiates in this [illegible]us disorder, and some valuable practical comments on the subject.

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voluntary aids which a patient can render to himself, when the respiration is laboring and distressed. In an aged person, for example, suffering under chronic bronchitis or catarrhal influenza —and gasping, it may be, under the difficulties of cough and expectoration—an opiate, by suspending these very struggles, often becomes the cause of danger or death. The effort here is needed for the recovery of free respiration; and, if suppressed too long, mucus accumulates in the bronchial cells, its extrication thence becomes impossible, and breathing ceases altogether.

This caution is not unnecessary. I have known more than one case, where there was cause to believe that the fatal event had been hastened, if not produced, by an opiate indiscreetly given at bedtime; either from anxiety in the practitioner himself to relieve obvious distress, or from the solicitation of those around that something should be done. Instances of this extreme kind are happily not frequent. But the medical man is bound to be prepared for them; and to have in readiness, not his judgment only, but his firmness and power of forbearance. For the feelings are often invoked, as well as the reason; and he has to balance a certain present distress against a doubtful and contingent danger. No technical rule can be laid down for such cases. Experience and the discretion of the moment, must alone decide what should be done, and what refrained from.

The employment of opiates in cerebral affections is another question of much interest and various difficulty. It might be conjectured that there are some cases where the benefit is great; others, where injury alone can result; and experience fully confirms this presumption. Though it would be easy to detail particular cases in which one or other of these results is likely to ensue, I know no single principle, yet ascertained, by which they may be classed and distinguished. Perhaps one of the best practical tests might be found in the state of the pupil; known to be differently affected by opium, by belladonna, and certain other narcotics. Where contraction of the pupil is one of the symptoms of the disease, it may with some reason be supposed that Opium is contradicted as a remedy; and a like inference might lead to the use of the opposite class of narcotics in its stead; transposing these methods, where the disease habitually produces a dilated state of the pupil. As far as my experience goes, it partially confirms the accuracy of the test, but is by no

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means sufficient to allow me to speak of it with safety. We are still obliged to act here upon the suggestions of the time and symptoms present, or upon the experience which partial trials may afford.

There is great scope for further research on this subject, as on all that relates to disorders of the brain; and a strong presumption that Opium is capable here of larger and more beneficial application than has yet been given to it. In certain cases of insanity especially, where much active irritation is present without inflammation, its employment (not by partial and irregular doses, but by keeping the patient for some time steadily under its influence) is often attended with a good attainable in no other way. Here, as in so many cases of physical pain and irritation from other causes, the maintenance of repose is a summary advantage; giving time and power to the functions of mind to resume their natural condition.[1]

The latter remark applies strongly also to certain states of fever, where the typhoid character has come on; and, with it, much disturbance in the functions of the brain, delirium, or intolerable restlessness. Here opiates may often be used with singular benefit, and without any of those ill effects which might be presumed from a hot skin, a dry tongue, suspended secretions, and disorder of the head. The removal of cerebral irritation, especially if sleep ensues, is a good which might weigh against many inconveniences, even if they occurred. But, in point of fact, they are much less likely to happen where the Opium directs itself specifically to the relief of irritation of this nature.

It is needful to add, that there are other cases of fever of typhoid kind, and where seemingly the same cerebral irritation is present, yet where opiates appear to act injuriously, by producing a comatose state rather than sound sleep. Unless distinction be found in the different state of the pupil, noticed above,—or in the hepatic symptoms which perhaps are more marked in these cases, and injuriously influence the state of the brain,—I have no practical test to recommend, beyond that careful observation of the first trial which may give guidance for the future. It is an important point in treatment, and deserves explicit attention.

[1] Dr. Seymour has well distinguished these cases; and made other valuable remarks in recommendation of Opium, in his treatise on these disorders.

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In many of these cases of disturbance of the brain, opiates may very advantageously be combined with the use of the tartarized antimony. And I would further observe, that morphia, in one or other of its preparations, may usually be preferred here to any other form of the remedy. Without entering into questions as to separate action of narcotine and other principles contained in common opium, I think it is proved that the effects of morphia are more explicit, more secure, and freer from injury or inconvenience, than those of any other opiate. It is in some sort the gain of a new agent in medicine; giving us not only less equivocal results, but also the power of a nicer adaptation of means to the peculiarities of each case. This is an acquisition of especial value, where the functions and disorders of the brain are concerned; and we have probably not yet ascertained all the good to be derived from it.[1]

The three topics I have thus slightly touched upon—the employment of Opium in inflammatory diseases; in disorders of the heart and respiratory organs; and in affections of the brain—involve some of the most difficult questions as to the use of this remedy. Many others would occur were the subject treated in detail; and particularly those which relate to its effects in disorders of the liver and alimentary canal. Here again there is need of much discrimination; and it may be affirmed that these are especially the organs, the action of Opium upon which is liable to most suspicion of injury or failure. Except in cases of simple morbid irritability or spasm in some part of the canal, where it becomes a fit adjunct to other means, its general and certain tendency is to impede or disorder all the processes of digestion, and the secretions therewith connected. And in complaints of the liver and its appendages I scarcely know one, unless it be the painful passage of a gall-stone through the ducts, where Opium may not be said to be hurtful in its separate effects, though occasionally and indirectly useful when combined with other means. I have alluded to affections of the brain in which the liver is concerned, in aggravating at least the symptoms;

[1] In like manner the discovery of aconitine and veratria provides us with other agents on the nervous system of great and singular power; and capable, in their chemical form, of much more definite use than under the previous method of employing these narcotics.

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and here too its influence is for the most part injurious; and always to be regarded with suspicion.

In those cases, generally, where a course of purgative medicines is distinctly needed for the relief of the portal circulation, or of congestions about these viscera, Opium can rarely be used without direct injury, or delay in the progress of cure. Where relief to irritation is required, it is better sought for in these cases by a temporary suspension of the active remedies in use; and the physician shows his skill by steadily maintaining the principle of treatment which he has seen cause in the outset to adopt. To tamper with it by the intervention of opposite methods, is for the most part a feebleness hurtful both to the patient and practitioner.

There is much greater difficulty in laying down rules for that large class of disorders to which the name of bowel complaints is applied;—some of them symptomatic of the more general states just mentioned; others, the effect of irritations or disease of the mucous membrane in some part of the alimentary canal; others produced merely by improper or undigested food. The second class of cases, including dysentery, is that in which opiates seem most frequently admissible; either conjoined with, or interposed between, the laxative medicines which are more obviously essential. There are cases also of peculiar morbid irritability of the stomach, which are best relieved by a few drops of laudanum, sometimes combined with an alkali, taken immediately before food. But it must be owned that the general course of treatment in all these disorders is vague and inconsistent; and that no divisions or descriptions have yet been adopted, determinate enough to define the proper direction of practice, or to supersede the particular judgment in each case. While seeing, however, in this and other examples, how far medicine is from being an exact science, we may recognize the ample scope it affords for sagacity and watchful observation, and the eminent value of these qualities in every part of practice.

The external application of Opium is not sufficiently brought into use; nor is there a due appreciation of all that may be attained in this way. That certain of its specific effects are conveyed through the skin to other parts of the body is unquestionable from experiment; though the action, so produced, is less strongly marked than that of belladonna and some other nar-

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cotics. These effects are distinctly augmented by removing the skin; so as to bring it into more direct action on the nerves of the part, if topical causes are concerned; or within reach of readier absorption, if more diffused effect be sought for.[1] In many cases of painful affection of particular nerves, I have found great benefit from a strong opiate ointment on a blistered surface; or a little of the muriate of morphia sprinkled over it. In certain obstinate cases of gastrodynia, very trying to the skill of the practitioner, though the cause cannot thus be obviated, yet we can often obtain much relief to suffering by similar means applied to the epigastrium, or other more especial seat of pain. I have found no remedy more beneficial in those irritable forms of local psoriasis, which are so distressing from their obstinacy, than the application of poultices, prepared with a small proportion of a solution of opium, and continued until the state of the skin is thoroughly changed.

In many nervous and spasmodic affections, as in some forms of asthma, when we have reason to suppose that the medulla spinalis is the chief source of disorder, the same remedies may be applied along the course of the spine; and often with singular good, when all methods of depletion or counter-irritation have proved utterly unavailing. If Opium might be supposed capable of good in hydrophobia, after the many fruitless trials of it, this is the manner of use most likely to succeed; diffusing its action over a large surface, and by the aid of rubefacients rendering the cutaneous nerves more sensitive to its influence. The general method of practice is one which I feel assured, on experience, may beneficially be carried much further than we have hitherto pursued it.

The resources of external treatment ought ever indeed to be present to the mind of the practitioner. If insufficient for cure,

[1] The experiments of Sir B. Brodie (*Phil. Trans.* for 1811), and the more recent researches of Müller, show that both these agencies of opium must be admitted, but render it certain that the action of absorption into the blood is much the most important in kind and degree. All modern inquiry tends to show the facility and extraordinary speed with which substances may enter into the circulation by means of imbibition, not before known to exist. Even since the remarks of this chapter were written, the Endermic method of treatment, as it is called, has acquired a remarkable extension and increase of repute; justified on the whole by present experiment, and sanctioning the hope of much future benefit from this modification in the use of remedies.

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they yet are adjuncts of great value to other more active means of treatment. But often the relief they afford is more speedy than that from internal remedies; and, what can never be too highly appreciated in medicine, with much less ambiguity of result. Cases indeed are related of serious mischief from Opium applied externally to wounds, or excoriated surfaces; but such instances are rare, and probably to be qualified by facts regarding the peculiar temperament of those so affected.[1] The more common cause of failure in this remedy is its insufficient use; either from the proportion of Opium being too small, or from the careless manner of application common to this with other external remedies. It is more difficult to secure constancy in the patient and in those about him, when the means are simple and obvious, than when hidden under more obscure formulæ for internal administration.

[1] I have seen one or two remarkable instances of the noxious effect of belladonna, thus applied, upon the sensorium. In one of these, delirium, continued for more than thirty hours, was the consequence of dressing a blistered surface, by mistake, with belladonna ointment.

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CHAPTER XXVIII.

ON MERCURIAL MEDICINES.

It may seem that any remarks at the present day, on the principle or methods of using Calomel, must be trite and altogether needless. Yet is this only in part true. One comment I would make is, that, in the ordinary use of this medicine as a mercurial, its good effects are often much impaired by admixture with other aperients. Such combination is doubtless most beneficial, or even necessary, where the object is to obtain large and speedy evacuation of the liver and bowels. But in the numerous cases where the proper mercurial action is desired, either on the mucous membrane of the intestines, or on the different secreting organs, or in arresting certain states of inflammation, Calomel will generally be found to act most beneficially, and with greatest certainty, when given alone. Its combination with purgatives in these cases both obscures and impairs its effects; introducing at the same time causes of irritation, which disturb the body in other ways, and thereby check the influence of the medicine, and the general progress of recovery.

The same remark holds good in many instances where the liver is the direct object of mercurial treatment, exception of course being made for the contingency mentioned above. Even in cases of obstruction of the bowels, when there is threatening of topical inflammation, Calomel adequately given, without the addition of other laxatives, will often be more effectual for relief than in any combination with them. Its separate action is much less irritating to parts in this condition; while there is generally more distinct and speedy exercise of its specific effect upon membranes already in an inflamed state.

To avoid irritation and pain, indeed, whenever this can be done without too palpable sacrifice of the objects in view, ought ever to be present as a principle in the treatment of disease. In

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all cases these are positive evils, counteracting or retarding more or less the good gained. In some habits their effects, long continued, are mischievous even to the extent of danger and death. I doubt whether this principle, which I have dwelt upon elsewhere, is sufficiently regarded in the ordinary course of practice. We too often see methods of treatment, external as well as internal, usurping the names of remedies, and capable of acting as such in some constitutions; but which in others, by repeated irritation, have fretted the body into new disease, more difficult and distressing than that which was the original object of cure. The proof here is the relief gained by intermission of the means; but it must be admitted that these are cases requiring judgment and discrimination.

When thus given in a simple form, and where there is no strumous disposition or idiosyncrasy in the habit, it is remarkable how much Calomel may be borne without injury or inconvenience; and even without any such urgent effect on the bowels, as might be supposed from its effects in ordinary use. That the medicine is often needlessly and injuriously employed, as are many others, cannot be denied. But there is an abuse also on the side of timid and deficient employment, where its decided and speedy influence is required. This is often forfeited by using it in those small and scattered doses which harass the bowels with fretful and irregular action, without being efficient for relief. If mercury be needed for its specific effect on the system, or even on a part of the body, the sooner, generally speaking, that it is brought into action, the better.[1] Or if exception be taken against this as a rule, its agency is at least important enough to warrant its being used singly and steadily; without the ambiguity of any combined means, other than such as may be wanted to support the system under its action, or to aid in directing it to any given part.

This consideration, of supporting the action of Calomel by simultaneous assistance to the powers of the system, is often

[1] This is a question of some interest in practice, and respecting which opinions will differ. Perhaps it may best be answered by reference to individual temperament, and the nature of the disease. My own impression is, as stated above, that when we seek to obtain the specific effects of mercury, it is for the most part expedient to get the medicine into the system as quickly as possible, with due regard to the other circumstances of the case; regulating afterwards the degree and duration of its action according to the symptoms.

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neglected; from some supposed incongruity, much greater than really exists, between its action and the functions which tend to repair or invigorate the body. In those cases where frequent repetition, or long use of the remedy is required;—or where the body, as often happens, is debilitated by disease, which yet needs the activity of this medicine for a chance of favorable issue,—the direct conjunction of Calomel, or other mercurial, with tonics, is frequently not only the safest, but even the most effectual way of using the remedy. In such cases, of which some dropsical effusions may be taken as examples, the mercurial action in producing absorption seems to be quickened and sustained by the addition of bark or steel; and where no fever is present, or irritation in the primæ viæ, these medicines may often be made beneficially to concur with every part of a mercurial course. I would willingly press this point as one of importance, and not duly admitted into practice.

There is some cause to believe regarding mercury, as with respect to opium, that in certain conditions of active disease, where its agency is most beneficial and nearest to what may be termed specific, the medicine is capable of these effects, without any such action on other parts of the system as would have occurred under different circumstances. The truth of the observation is best attested in cases where it is given to subdue inflammatory actions in serous membranes; such inflammation as tends to effusion, unless speedily overcome.[1] May it be supposed that this arises from a more express direction of the mercurial action to the kidneys in these and similar instances? It is known that there are certain states of organic disease of the kidneys, in which mercury produces its constitutional effects more rapidly than usual. The fact, which is an interesting one, would seem to illustrate the supposition just made; and by showing the intimate relation of mercurial action to this function, will explain other anomalies in its effects on the system.

The remarkable influence of mercury in subduing certain inflammatory actions, both acute and chronic in kind, but particularly acute inflammations of serous membranes, is best explained

[1] I find it justly remarked by Dr. Seymour, in his Treatise on Dropsy, that the quantity of mercury which may be beneficially used in cases where inflammation, ending in deposition of lymph, is going on in the pericardium, is almost inconceivable; and this without affecting the gums or producing other inconvenience.

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by supposing its action to be on the capillary vessels, whether of membranous or glandular organs. Without assuming to define what is the particular state induced on these vessels as opposed to inflammation, I have always found this idea (long adopted by many eminent observers) to furnish the best clue to the various actions of mercury on the system. Its topical direction to particular organs (if indeed we rightly so interpret what in many cases may be only the different tendency in these organs to show the influence upon them) is subordinate, as may fairly be presumed, to this more general principle. And though it is difficult to understand how it should be a cause of specific inflammation in certain parts, compatibly with the other actions just stated, yet is this difficulty but part of our ignorance of the general theory of inflammation, attested still by so many differences both in opinion and the statement of facts. The whole subject is interesting, from the connection it indicates between the action of a very powerful medicine, and the functions of that part of the vascular system which is now rightly looked to as the principal source and seat of disease.

I believe that a mistake is frequently made as to the manner of operation of Calomel and other mercurials, in ascribing effects to their action on the liver, which are in fact chiefly due to their influence on the mucous membrane and glandular follicles of the intestines. The action, prompt and almost specific, of mercury upon the liver, cannot be doubted; but scarcely less in amount or importance is its effect upon the parts last mentioned. The secretions and discharges thence arising, particularly from the mucous membrane and glands of the colon, are often mistaken for those from the liver; and the treatment continued and pressed forwards upon this supposition. In many cases there may be no practical evil in this; in some it may even prove beneficial, mercury being the fit remedy whatever the seat of the disorder. But in other instances the error is certainly of mischievous tendency; and at all events we are bound to seek for what is true in knowledge as the basis of our practice.

In the employment of blue pill, as well as of calomel, I believe that the benefit is often lessened by combination with more active aperients; and this is especially the case, where it is used as an alterative for disordered secretions, or in certain cases of dyspepsia. In these instances it will generally be found more

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advantageous to give it twice or thrice a day—in moderate doses and uncombined—than to limit it, as is usual, to one larger quantity at bedtime, with some purgative combination. I cannot affirm that I have seen any distinct effect from the very minute doses of the blue pill, recommended by some physicians; but I do not doubt from observation the benefit of small quantities, frequently repeated in the chronic cases just mentioned. This mode of using mercurial medicines is common in the complaints of children, but by some incongruity less frequently applied in those of adults. There is certainly no practical reason for the distinction, unless it be the seemingly greater power of resisting the mercurial action in the former; a point itself liable to some doubt.

I have no design here to enter into details as to the action or manner of use of Calomel in different diseases. This is too large a subject to be otherwise considered than in its relation to each. If I might venture a general comment upon the present employment of the medicine, it would be that it is too frequently used as a mere laxative remedy; and too sparingly and inefficiently, where relief to certain varieties of inflammation is sought for—or the absorption of dropsical effusions—or the separation from the system, principally through the liver and the secreting surface of the intestines, of certain morbid matters which, under one denomination or other, are largely thus excreted from the body. And I would repeat that in pursuing some of these objects, particularly that of subduing inflammatory action, it is often much more expedient to give it alone than in any form of combination. The question of salivation is of less moment here than it might on first view appear. If the mercury, thus singly given, passes more readily into the body, it does for the most part accomplish more speedily the object sought for. And in the cases of exception to this, and of anomaly in the action of the medicine, the particular circumstances generally furnish best direction as to what should be done.

Though most of the beneficial effects of mercury may be attained, short of that constitutional action which is testified by salivation, yet there are certain cases where it seems that the more complete ingress of the medicine into the system, thus testified, is followed by greater and more lasting benefit, than when the effect on the mouth has not been obtained. I think I

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have observed this most frequently in cases of hepatic disease; but the fact is undoubtedly true in others also. Salivation occasionally, and in certain habits, becomes a serious evil; but its earlier stage is generally so much under medical control, that no valid objection to the remedy can be thence derived.

While commenting upon the ordinary manner of use of mercurial medicines, I may notice what I think to be a needless degree of apprehension, not with patients only, but even among medical men, as to exposure at the time of taking them. In every case where purgative action is sought for, whatever the means employed for this purpose, it is right to guard against cold or other causes depressing the body, or creating sudden changes in the balance of circulation. The effect of this action in producing at the time a certain collapse of power is a sufficient motive for the general caution. But there is nothing so especial in the case of mercury thus used, as to call for its relinquishment, when other means are fairly admissible for the object. Or if it be so in certain instances, I cannot doubt that the apprehension is carried much too far in others, thereby altering frequently the course of practice where no such change is required. I would not willingly withdraw any caution really salutary; but medical practice ought not to be fettered by traditional opinions, or the current and careless phrases of society.

The dread prevailing in France and Germany as to the use of Calomel, and the reprobation of English practice on this score, are well known. Unless partial cases of abuse be admitted as argument, such as may occur in the case of any other medicine, it cannot be allowed that there is much justification for these harsh views. It may, indeed, be affirmed (and I state this on my own observation), that the judgment is formed mainly on hearsay, and has gained weight by mere repetition. The actual knowledge from experience of the medicinal qualities of calomel is, on the Continent, exceedingly limited; yet this is the only evidence which can fairly be brought to the question. Who, for instance, that has not seen the effects of the medicine, duly given, in severe cases of croup—or in certain affections of the brain—or in cases of great congestion about the liver and abdominal viscera—or in particular cases of membranous inflammation,—can rightly estimate the benefits procured from it, and procured, we are often able to say, without any injury to coun-

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tervail the good? It is needless to multiply examples to this effect. The experience of every medical man in this country will supply them; but without such experience no censure like that alluded to can justly be applied.

It is of course not to be inferred that these results are derived from the use of Calomel alone. Where the peculiar action of mercury is the main object, other preparations may be equally capable of fulfilling it; and in some cases this object is perhaps even better attained by the form of mercurial medicine of which I am about to speak. But the more explicit knowledge we possess of the effects of Calomel, and its undoubted sufficiency for all the most important purposes of practice, must ever sustain its repute; whatever equivalents may hereafter be acquired for the results we now obtain through its agency.

The error of many Continental physicians on this subject arises, I believe, from their connecting the large use of purgatives in English practice with the idea that Calomel is the medicine chiefly employed for this object. I have spoken in another place of what I think to be the real excess in this part of our practice; and, as respects Calomel, have allowed that it is often needlessly—sometimes, and in particular temperaments, injuriously—employed. But the admissions ought not to go further than this; and there remains to all who rightly estimate its use a large balance in favor of the medicine, fully sanctioning the place it holds among English physicians in the treatment of disease.

Among the different mercurial preparations, the Oxymuriate, or Bichloride of mercury, is not so generally used as it merits to be, though doubtless more extensively now than heretofore. Under discreet employment it is one of the most valuable remedies we possess. The power of giving it in solution is a material advantage in various parts of practice; rendering its action more certain, more equable, and probably, by readier absorption, more effectual as an alterative upon the whole system. I have seen its influence in augmenting and improving the secretions—procuring the absorption of morbid growths—altering the state of the skin in many cutaneous disorders—and changing the character of morbid actions generally throughout the system—in cases where I believe no other medicine, or combination of

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medicines would have had equal effect. Its conjunction with bark, steel, sarsaparilla, &c., affords resources of the greatest value in the treatment of disease. And though otherwise held by common opinion, I think it on the whole as safe a medicine as Calomel in the hands of the practitioner; inasmuch as its distribution can be made as equal and determinate; and its effects, from being given in a state of solution, are less likely to be interrupted by mechanical hindrances in the stomach and bowels.

It is worthy of note, indeed, how long this medicine may be continued in uninterrupted use, without obvious injury or inconvenience. I have often given from four to six drachms daily of the Liquor hydrarg. bichlorid. (*Pharm. Lond.* 1836) for six or eight weeks; without affection of the gums, irritation of the bowels, or any effect making it needful to intermit or modify its use. In other cases this is not equally possible: but the difficulties which occur from idiosyncrasy, or other causes, are quite as readily surmounted here as in the case of any other mercurial, and are rarely such as to prohibit the use of the medicine altogether.

Perseverance in the use of this preparation is, according to my experience, of singular avail in certain cerebral or spinal disorders; where there is presumption that some effusion may have taken place, or that the state of obscure inflammation exists, tending to this effect. I have seen great benefit from it, thus steadily employed, in several cases of paraplegia; the usual slow progress of the disorder giving ample scope for its effects, and the danger in prospect justifying the full trial of the remedy.[1]

[1] Adverting to this disease, I may remark that in those cases of it which appear (more perhaps in the results of practice than from any assured diagnosis) to be unconnected with atrophy or other change of the nervous substance itself, I know no remedy which can compare with mercury *adequately* used. One of the most remarkable cases I have seen, attesting this efficacy, was that of a patient about forty-three years of age; where the symptoms, beginning with weakness of the lower limbs and loins, preternaturally slow pulse, and slight confusion of mind, proceeded gradually in the course of a few months to total inability in these limbs, loss of power in the sphincters, extreme difficulty of articulation, failure of memory and intellect, and general want of nervous power throughout the whole system. Under a treatment of full doses of calomel twice a day for about five weeks, and then once a day for an equal time, without intermission, yet never proceeding to actual salivation, the symptoms were all progressively removed; the restoration of the voluntary powers and intellect being best proved by the patient regaining his

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In other instances where the object has been to procure absorption of a deposit or thickened texture, or to change the condition of action leading to this, as in particular cases of hypertrophy, I have found equal facility in sustaining its action for a long period, and this without obvious injury to the system.

I may repeat generally my persuasion that much advantage might be gained from the larger and more various employment of this mercurial remedy. But to obtain the full benefit, we must be patient as well as decided in its use. In those cases where it is of greatest avail, the processes of change are often slowest; and not testified by those instant and obvious results, which are sometimes needed to fortify the mind even of the physician in the perseverance proper to the practice—still more to satisfy the patient and those around him; alarmed, it may be, by the name of the medicine, and by the precautions taken as to its dose and effects. As a result of these or other difficulties, I have repeatedly known a course to be abandoned in the middle (sometimes at the very moment when becoming most effective), which there was every reason to believe capable of relieving eventually a distressing or dangerous malady. Such difficulties are best obviated by a just anticipation—and, it may be, declaration—of what is to be done, and what expected; and by a steady recurrence to the same principles throughout the whole progress of treatment.

In these few remarks on mercurial medicines, as on so many other subjects in this volume, I have referred to those points only which seem to deserve further consideration than they have hitherto received, or a more direct and certain application to practice.

power of walking and riding, and by being able to preside again as Chairman of the Quarter Sessions. The mercury was used in this case almost to the exclusion of other internal remedies. The only means, which could render the result ambiguous as to cause, were two issues kept open over the cervical and lumbar vertebræ. But, though these had doubtless some effect, the relation of date between the remedies and the relief obtained gave proof that the mercury was of greatest avail.

I ought to mention, that life was terminated half a year after his recovery by a sudden apoplectic seizure, the effect of some over-exertion in country sports; an event not invalidating the inference as to treatment, though showing that the same tendency still existed in the habit. No post-mortem examination took place.

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CHAPTER XXIX.

ON THE USE OF DIGITALIS.

After all that has been written upon this medicine, there is reason to doubt whether its capabilities have yet been brought into full use. An agent which can thus alter the heart's action, and, under management, safely sustain this alteration for a considerable time, is a very important one in the hands of the practitioner; affording means wherewith to control, more or less directly, all the actions of the system. It is, however, this singular sedative effect on the heart, and the occasional suddenness of the change so produced, which creates an apprehension as to its use, not warranted by what we learn from exact observation. Though employing the medicine somewhat largely in practice, I do not recollect a case in which I have distinctly seen injurious consequences from this cause;—none such, certainly, as were not speedily relieved by its discontinuance, and other means of easy adoption.

I am far from inculcating neglect of the symptom just mentioned. But while admitting that deviation below the average standard of the pulse is less frequent and considerable than excess above it, and that certain morbid states of the brain are sometimes thus indicated, I feel assured that alarm is too readily taken in the early effects of Digitalis on the heart, even should intermittent action be one of these. Similar effects occur habitually from so many other causes affecting the body without injury which can be especially referred to this symptom, that if there be justification on other grounds for sustaining the action, and care given in watching it, the practice may generally be pursued without any risk sufficient to countervail the good.

More attention too is required, than is always bestowed, in estimating the actual changes in the pulse. It is not enough to

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feel it once, or in one posture only. The difference between recumbency and the upright posture will often totally alter its character; and it should be examined also after some cordial has been given, to ascertain the facility of bringing it back to its ordinary state. These and other precautions, by better determining the real amount of deviation produced, afford securities for the continued use of the medicine which are of great practical value.

Such suggestions regarding the pulse apply to many other cases, where we seek to know the effects of disease upon it; and not, as here, the influence of the remedy. The value of its indications is often forfeited by the slight and careless manner in which they are sought for. An inference may be formed at one moment, or under one posture, which the lapse of five minutes, and change of position, will altogether belie. This is less the case in fevers and inflammatory diseases; but there are many others where the view of the disorder, and method of treatment, may be wholly perverted by trusting to a single observation. All recent inquiry into the pulse shows the need of attention to these points.

The manner of employing Digitalis is undoubtedly of much consequence. I feel assured, on experience, that the combination with tonics and stimulants not only does not impair some of its specific effects, but even extends them by giving larger scope to its administration. This may be inadmissible when a direct sedative effect is required (though here, as in the case of Dr. Ferrier's original suggestion of Digitalis and iron in phthisis, the line cannot always distinctly be drawn); but when the action of the kidneys is especially sought for, and in habits already debilitated by disease, such combinations are often of the greatest value. That with iron seems to me more beneficial in many cases than with bark; nor producing any mischief or ambiguous effect from the seeming incongruity of the two agents. I have frequently given Digitalis in the ordinary doses, for weeks together, in combination with various forms of steel, and especially the Tinctures, without finding cause to withdraw or intermit the medicine; and this in cases where the patients were far advanced in years, and the circulation becomes feeble from disease. It would be difficult to find any single combination more effective than this in old cases of general dropsy; in œdematous

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swellings from debility; or in the anasarca following scarlet fever, where, together with weakness, there is still left an excited and irritable state of the arterial system. The action of the foxglove, as a diuretic, seems even to be quickened and sustained by being conjoined with iron; while the latter acts apparently as a safeguard against some of its peculiar effects.

It cannot be doubted that this medicine may often be given with safety and advantage, even where the action of the heart is already extremely irregular. The irregularity arises occasionally from causes which the Digitalis is capable of lessening or removing; as from fluid actually effused. But, besides this less direct influence, it is probable that its immediate action on the heart, tending under certain circumstances to produce intermittent pulsation, may under other circumstances correct the irregularity already existing. And accordingly I have often found perfect equality of beat to ensue for a time upon its use, where the motions of the heart had been very unequal in their ordinary state.

The effect here is the same as that which frequently occurs from fever, or other cause of excitement to arterial action, in rendering regular a pulse habitually intermittent; but the fact is more remarkable in the instance of Digitalis, from its peculiar influence on the heart under ordinary circumstances. We have reason, indeed, to suppose at least two different states of this organ, independently of structural disease, in which intermittent action may occur, from causes almost the converse of each other; and in this probably lies the explanation of the seeming anomaly in question. The point is a curious one in pathology, and meriting notice from the inferences it suggests.

And here I am led incidentally to remark that a treatise on the various irregular actions of the heart, *unconnected with organic causes*,—and defined as such,—would, if well executed, be of great practical value. The success recently attained in deciphering by external signs the various structural changes of this organ, leaves too much in arrear the knowledge and classification of the many other causes which disturb its action, and often assume every aspect of real disease. To those especially who have not had time or opportunity to reach the needful exactness in auscultation (and partial knowledge here is often more dangerous than ignorance), it is of great moment to know the different causes

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which may render their first impressions ambiguous. It is no impeachment of the true value of such methods, to state that frequent mischief has been done by making them the sole interpreters of disease; without due regard to those other numerous causes of disturbance—mechanical, chemical, and nervous—which are ever tending to affect and disorder this great centre of the circulation.[1]

Recurring to the subject before us, I may add that I have often employed Digitalis beneficially, in cases even where there was proof from auscultation, or otherwise, of the heart being actually diseased in structure. There is need here, however, not only of much watchfulness, but of discrimination as to the kind of disease. The enlarged and flaccid heart, though on first view it might seem the least favorable for the use of the medicine, is perhaps not so. At least we have some reason to believe that in the dropsical affections which depend on this organic change, the action of Digitalis, as a diuretic, is peculiarly of avail. Generally speaking, in all cases where there is obvious mechanical difficulty, valvular or otherwise, to the passage of the blood through the heart, so as from slight causes to give to the patient apprehension of fatal stoppage, we are bound to be cautious, lest, in seeking to relieve quick and irritable pulsation, we should bring on some worse alternative.

The action of Digitalis seems, indeed, peculiarly the subject of

[1] I should depart too far from the subject of this chapter, were I to do more than simply enumerate in a note the chief irregularities in the heart's action which may, and do, occur transiently from various causes, independently of disease of structure in the organ itself. These are—alterations of rate of action, whether by acceleration or retardation; alteration in the strength of beat, with or without change of rate; deviations from the equality of rhythm, either by unequal intervals of time, or unequal strength of successive pulses; complete intermissions, either at regular or irregular intervals; the longer cessation of action in the fainting fit; the jarring irritable action, without change either of rate or regularity; and that condition best described by the term of *laborious actions*, easily passing into inequality of rhythm, and power.

Other slighter irregularities might be named; but rather with the effect of encumbering than aiding the young physician, amidst the difficulties which beset this part of practice. Of these now mentioned many—perhaps nearly all—may occur in connection with, or in effect of, organic disease of the heart. But it is important to know that they may all happen, in one degree or other, from temporary causes, and without any obvious or permanent change of structure. The practical value of discrimination here, for the welfare of the patient, and the credit of the physician, cannot be too forcibly dwelt upon.

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anomaly throughout; and I am partly led to these remarks, as pointing out the void there yet is in our knowledge of some of these very powerful physical agencies on the body. Scarcely even is the question wholly settled, whether it is directly, or only indirectly, sedative. The view of its action as one of accumulation in the body, and sudden effect, is still uncertain;—or if thought to be determined in fact, liable at least to doubt in the practical inferences drawn from it. Another source of difficulty is the doubtful relation between the sedative and diuretic effects of the medicine; understanding by the former term, its peculiar influence on the heart and circulation. This connection, though doubtless existing, and probably an integral part of its action, has not yet been rightly ascertained. In following the inquiry, there are certain points which occur in the outset, as essential to it; such more especially as the question, whether the diuretic action of Digitalis is upon the kidneys immediately, or intermediately through changes it produces on the vital actions of the sanguiferous system? In answering this question (which may probably be done by affirming the latter opinion), we are called upon to refer to the fact, that it is chiefly in dropsy of the cavities of the chest, and anasarca, that its action as a remedy is of any avail. Pursuing this observation, it becomes likely that most of the doubtful or anomalous effects of the medicine will hereafter be found to resolve themselves into its influence, direct or indirect, on the heart and different parts of the circulation.[1] The recent experiments of Dr. Traube, of Berlin, by injection of the infusion of Digitalis into the jugular vein, while confirming some of the results before obtained by Magendie, have illustrated other curious anomalies in its action on the heart, depending on the quantity given, and the speed with which it is taken into the system. These experiments further show the remarkable effect of the division of the pneumogastric nerves, in obviating or annulling the influence of Digitalis on the pulse.

Much, however, remains to be done before we can confide in our knowledge of this singular medicine. Organic chemistry has of late been actively at work here, as elsewhere; and MM. Homolle and Quevenne have obtained by analysis fourteen differ-

[1] I have never seen an instance of ptyalism produced by Digitalis; but the case recorded by Dr. Henry, and some earlier evidence of the same kind, make it probable that this is an occasional consequence of excess in its use.

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ent substances from the leaves of foxglove; to one of which, the *Digitaline*, the specific qualities of Digitalis appear to be chiefly due. In this respect, as in others, there is a close analogy to the chemical constitution of opium and other vegetable narcotics; and there is reason to hope that by the same process of elimination as applied to them, we may bring the active principle of Digitalis into a form of use more simple and certain in all its effects.

Until we thus, or through other means, acquire more exact knowledge of the mode of action of this medicine, we cannot look to obtain all those increased advantages in practice, which may be anticipated from its peculiar agency on the circulation. We must be content to proceed empirically, as we have hitherto done, in noting its effects in particular cases, and under given combinations. In proof of the present need of submitting all theory to practical observation, I may mention that though convinced of its value in combination with bark or preparations of iron, in dropsical cases, yet I am led by experience to acquiesce also in Dr. Withering's opinion, that its effects as a diuretic are often greater in states of feeble circulation and relaxed fibre, than where the habit is more vigorous and firm. Nor can I doubt, from observation, the truth of Dr. John Davy's remark, that patients weakened by disease often bear much larger doses than those in sounder health. Admitting the facts to be severally so, we must refer for explanation of the seeming contradiction, to the manner of action of Digitalis on the vascular system, still so imperfectly known; and to analogy also in the case of mercury and some other medicines, where a certain state of the circulation is required to give specific direction to some of their effects.[1] We know too little, moreover, of the manner of action of iron and bark, to be able to affirm that they would prevent this state being attained; even though commonly using these medicines for purposes apparently of opposite kind.[2]

The uncertainty of diuretic medicines in general is, indeed, a

[1] The experiments which prove that the facility with which poisons produce their effects by absorption, varies according to the quantity or dilution of the circulating blood, may be considered to bear directly upon this point.

[2] In illustration of this remark upon our doubtful knowledge even of the medicines most familiar in use, I may refer to the recent experiments of Briquet (fully confirmed by Dr. Bence Jones) on the effect of large repeated doses of quinine in reducing greatly the frequency of the pulse, and depressing the vital powers.

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point where medical practice is greatly at fault. Scarcely can one be named on which we may uniformly rely; and the inequalities of action are rarely to be assigned to any known cause. As, however, they belong alike, or nearly so, to all these medicines, whether of mineral, vegetable, or animal kind, we must seek their origin not in the substances themselves, but in the functions of the kidneys;—liable beyond all others to unceasing changes; and from causes and combinations so various (interpreting, as they do, the state of every other organ and function of the body), that we may readily understand how medicines directed to them should partake in the same uncertainty.

It is among the remedies of this class, perhaps more than of any other, that combinations have their greatest value. Mercury, Digitalis, squill, cantharides, diuretic salts, and the different vegetable infusions having similar action, are severally capable of being aided in this way, either by conjunction mutually, or with other agents.[1] It must be admitted that the results so derived are purely empirical in kind—not explicable when attained, and subject frequently to the same failure as when the same medicines are given uncombined. Nevertheless, these are the proper objects of diligent observation; and the ability of the practitioner is in no way better shown than in removing whatever may render his conclusions equivocal or obscure.

Intending these only as general remarks on the action and application of Digitalis, I do not add to them by details of its employment in particular diseases. I would merely repeat my persuasion that its use as a sedative to the circulation, in diseases of inflammation or excitement, might beneficially be much extended; superseding depletion in some cases, the use of narcotics in others. Regarding its employment in pulmonary consump-

[1] In the instance of other diuretics, as well as Digitalis, it would seem that the direct conjunction with stimulants often adds to their efficacy. Ammonia, for example, independently of its own occasional action upon the kidneys, will often give to other medicines more certain and speedy direction to this organ.

In another chapter (On the Uses of Diluents) I have mentioned the value of these in their action on the kidneys, even in some dropsical cases; and it is well worth while to keep in mind such possible aid to diuretic medicines, when failing in their immediate and proper effect. It may be from disregard to this, that various popular remedies have less effect in regular practice than under older and more familiar methods of employment.

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tion, I am unable to add anything to the ambiguous recommendation it has heretofore received. It clearly has no power over tubercular deposits; and the only influence to be understood in such cases is that of abating the rapidity of circulation, where greater than is natural; an effect of less real moment than is generally supposed. In this, as in many other instances, the importance of a particular symptom as a test leads to a wrong estimate of its relations to the actual disease. We think ourselves on the way to cure, when merely lessening one effect of a specific disorder upon the natural functions of the body.

Respecting the several forms under which Digitalis is used, there is some reason to believe the infusion, rightly prepared, to be the most certain and beneficial in its effects, and admitting of every combination with other medicines which can in any case be required. This, indeed, is an instance where, from the nature of the agent, it is singularly needful that the quality of its preparations should be equal and uniform. It is certain that many of the inequalities and seeming anomalies in the effects of Digitalis are owing to neglect of this important precaution.

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CHAPTER XXX.

ON ANTIMONIAL MEDICINES.

I ANNEX this subject to the preceding, having chiefly in view what is still the insufficient use, in English practice, of those medicines expressly sedative to the actions of the circulation. I have noticed this in the instance of digitalis; and it is equally true as regards some of the antimonial preparations; and the tartarized antimony especially. The opinion held in relation to the supposed sudorific effect of these medicines, has prevented due attention being given to their influence in allaying inordinate action; either of the brain, as in mania and delirium of different forms; or more generally in inflammation, or febrile states of the whole system. In France, and still earlier and more extensively in some of the Italian schools, the use of emetic-tartar is well known to have been carried much further than in England, and under systematic views, as a contra-stimulant, which we scarcely yet fully recognize. The appreciation of its power is now, indeed, becoming more just and complete; but the progress is slow in relation to the assured benefits which may be derived from it in ordinary practice.[1]

In another chapter I have noticed the uncertain views which determine our employment of antimonials as sudorifics; and the

[1] The writings, as well as practice, of Professor Tomassini of Bologna, may be cited in proof of the extent to which the use of tartar-emetic may be carried; and also for the best account of the doctrines of the new Italian school on this subject, to which the names of Rasori and Borda, as well as his own, have given reputation. The work more recently published by M. Lapelletier, "De l'Emploi du Tartre Stibié à haute Dose dans le Traitement des Maladies," affords a complete history of all that is known on the subject.

I have elsewhere referred to the very curious account, in the letters of Guy Patin, of the violent struggle in France against the introduction of antimony into practice, during the early part of Louis XIV's reign.

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probability that any such effect from them is due chiefly to their influence in abating the circulation and the febrile state. To this may be added a reasonable doubt regarding the efficacy of some of the antimonial preparations most frequently in use. My own experience suggests the conclusion which has occurred to others, that the Antimonial powder of our Pharmacopœia is perfectly inert in any ordinary dose; and that the original James's powder has held a reputation barely justified by any method of its use; certainly not by the trifling quantities in which it is now given. All the gain we derive from these medicines as sudorifics or otherwise, is obtained far more largely, and with much greater certainty, by use of the emetic-tartar in similar cases. This, in fact, is the preparation of Antimony which, under proper management, may be made to supersede any other; and which, whether considering its power or variety of application, may fairly rank amongst the more valuable remedies we possess.

Of these applications, that which depends on its sedative effect is perhaps the most important. Without referring to any theories of this particular influence, or the mode in which it is exercised in the living body, it is enough to know that there is the power, directly or indirectly, of diminishing excess of action; and limiting thereby the degree, if not also the duration, of those conditions in which disorders of acute inflammation or inflammatory fever mainly consist. The reluctance shown by many to admit the phrase and idea of a *direct sedative*, may depend, in part, on too exclusive attention to the fact that augmented action is always followed by proportionate loss of power; becoming in this way the most frequent and familiar cause of sedative effect which is known to us. But the full admission of this general law by no means precludes the allowance of other actions upon the body, or certain of its functions, producing, at once, the effect which indirectly results in the other case. Abstractedly considered, the notion of any part of vital action reduced below its average or natural state, is quite as intelligible as that of the same action raised above it.

As a febrifuge, in the most distinct sense of the term, it may be doubted whether we possess any more direct, or speedy in effect, than Antimony under this form. It is impossible to have witnessed its effect, when adequately given, in acute pneumonia, or bronchitis, or croup, without recognizing the same fact; and

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seeing, that where it does not wholly supersede bleeding as a prior remedy, it at least abridges the demand for this; and comes in aid of it, by inducing the same state of the vascular system throughout the body. Its relation to mercury in these cases is that of most interest; the effect apparently the same, so as in many instances to make it doubtful which of the two remedies may best be relied upon for instant use; yet the mode of operation presumably very different.[1]

The evidence of Laennec, Louis, and other eminent French physicians, on many of these points is more explicit than ours; as founded on larger use of the remedy, and more direct comparison of the cases of pneumonia, so treated, with those submitted to other means. I know not that its value has been equally attested in other forms of inflammation; but sufficiently so, and especially in affections of the brain, to show that the sedative action is not limited to one organ, but extends generally to the whole system.[2]

Though the discussion as to the relative merits of the treatment by bleeding, and that by antimonials, in pneumonia, is not without use, yet it has perhaps placed the remedies too much in seeming opposition to each other. They come, in fact, in common aid towards the same object, and are often needed in conjunction;—the bleeding first, in such cases, for more instant and powerful effect, and possibly also as giving more power to the action of the tartar-emetic—the latter as sustaining the effect of the depletion, and often preventing the need there might otherwise be of its repetition. When the inflammation is slight, its relief may frequently be trusted to the antimonial alone. This appears to be the view by which the

[1] Dr. Stokes, in his work on Diseases of the Chest, remarks that the previous use of tartar-emetic in pneumonia seems to facilitate the further action of mercury in subduing the inflammation. He advises that in typhoid pneumonia the mercurial treatment should be pursued in preference to that by antimonials. What I have seen would lead me to concur in both these points of practice.

[2] The experiments of Sir B. Brodie, in the second of his valuable papers on the Action of Poisons (*Phil. Trans.* 1812, p. 205), render it probable that the brain chiefly is first acted upon by emetic-tartar so given; but with some evidence of direct action on the heart also. The suggestion of Magendie, that its fatal effects are due to an inflammatory state of the whole system, supervening upon absorption, does not seem warranted by any express fact; and is made less probable by the frequent absence of all inflammatory appearances on the coats of the stomach and intestines in such cases.

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relative employment of the two remedies may best be guided in practice.

I infer from my own observation, what is amply proved by that of others, that the sedative effects of antimony under this form are independent of all evacuation from the body. They may occur where there has been neither vomiting nor purgative action; and where perspiration, if happening at all, comes rather in sequel and effect, than as cause of the changes produced. We must consider the vascular system, either directly, or through the influence of the nervous system, to be the part concerned in, and the subject of, these changes. Some ambiguity exists here, as to the nausea brought on by the use of the medicine; and the relation of such effect to its sedative agency. It is not always easy to separate these effects; which seem, indeed, to depend concurrently upon the same cause acting on the nervous system. But there is enough to show the sedative action to exist independently of this, in the fact that it occurs where the nauseating effect has either not taken place at all, or been removed.

It must be allowed that there are many singularities in the influence of this medicine as an emetic, and that we yet only partially understand the conditions which so variously modify this quality. The mode of administration is, doubtless, in part concerned; and particularly as regards the quantity of the fluid vehicle in which it is given. The state of the stomach itself is another of the conditions. But there is also an influence derived from the general state of the body at the time; attested, as I think, in the case of fever; where, according to my experience, there is less effect from given doses in producing sickness, than when this state is absent. The same conclusion seems to be justified regarding its employment in inflammatory disorders. And further, it is manifest that a habit is acquired as to the medicine, lessening or wholly removing effects that have occurred on its first use; so that vomiting or nausea, very harassing in the outset, may cease to disturb the patient, even though the same doses are continued as before.

The influence, indeed, of dose and manner of employment, curious and important in all cases, well deserves notice here. Our practice in England (limited, as I have said, by older views) seldom goes beyond such use of them in inflammatory disorders,

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as to sustain nausea for a time, and thereby repress the circulation; and even this intention is often subordinate to the view of forcing perspiration, as a means of relief. It is certain that the full value of the medicine is not thus attained; and though we yet want proof of the efficacy or need of the large doses of emetic-tartar employed by some of the Continental physicians, it seems certain that those we use might beneficially be increased in relation to their sedative effect.[1]

The best test we have on this point is to be found in the objects for which we adopt the treatment; viz., the diminished action of the heart and arterial system, or the removal of some active irritation of the brain or other organ. In looking to this test, as well as on other accounts, it is very generally expedient to give the medicine under its simplest form. With a powerful agent of definite purpose in our hands, combination ought rather to be regarded as an exception than a rule of employment; enfeebling, for the most part, the principle of treatment, and perplexing its results. I have elsewhere alluded to this maxim in practice, which, admitting all the needful qualifications, is still important enough to warrant a constant regard to it.

In the instance of emetic-tartar, its combination with opium is undoubtedly that which most merits notice. This seems a fair example of a compound acquiring powers, which do not equally belong to either of its component parts. Its value is attested by the observation of many physicians, particularly in cases of mania and cerebral irritation; and my own experience entirely concurs in this respect. In cases more strictly inflammatory in kind, whether pneumonia, bronchitis, rheumatism, or inflammation of the membranes of the brain, the same combination is often useful; but under more reserve, both as to the period of the disorder, and the propriety of bleeding as a previous remedy. The difference of opinion respecting the fitness of combining opium with the antimonial in such cases, arises probably from the different attention given to the latter points. Whether with or without emetic-tartar, it cannot safely be em-

[1] The doses of those French physicians, whose practice is best authority (one or two grains, so repeated as to extend from 5 to 20 grains in the twenty-four hours), may be assumed as affording every scope to the beneficial use of the medicine in different cases. The maximum quantity here little exceeds what Rasori sometimes gave for a single dose.

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ployed where bleeding is required. But allowing for its unfitness in these instances, I still believe the harm done to be less, than were it given, under the like circumstances, not so combined.

Reverting to the use of this combination in affections of the brain, it is remarkable how speedy and assured the effect often is. In the wild restlessness of Delirium-tremens this is sometimes very strikingly seen. In fevers attended with much cerebral disturbance its employment has been attended with similar good.[1] And in various forms of spasmodic disease I have experience of the same kind; giving evidence of benefits which are not so readily, or completely, obtained by the use of opium alone.

It is probable that other applications of the tartar-emetic in the cure of disease will hereafter be ascertained; depending, it may be, upon other modes of agency than that to which I have chiefly alluded.[2] For though employing the term *sedative*, thus far, in the most simple and practical sense in which we can understand it, it is obvious that much theory attaches at present to the subject. Sedative action, so understood as merely opposed to stimulant, may occur not only from different agents, but in different ways as regards the organs first affected; and with varying influence on different parts of the system. The agency of the hydrocyanic acid (an important addition to this class of medicines, and one of the most explicit examples of their action) is evidently not the same as that of digitalis or tartarized antimony; and we have equal reason to suppose differences between the two latter medicines, seeing the diversity of their collateral effects. Though greatly instructed by modern inquiry in the action of poisons, yet is our knowledge likely to be much augmented hereafter in all that concerns their relation to the vital properties of the blood. And while thus approaching nearer to the solution of one of the most curious problems in physiology, we have the assurance of great aid in the treatment

[1] In a valuable paper by Dr. Graves (*London Med. Gazette for July* 8, 1837), this physician recommends the occasional use of emetic-tartar by enema: stating its effects to be the same as given by the mouth, and rightly commenting on the neglect of this means of administering various other remedies.

[2] In a late paper by Dr. Gemelle (*Bulletin Général de Thérapeutique, Mars*, 1838), cases are given of the successful treatment of synovial affections of the joints by the internal use of tartar-emetic.

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of disease; by better determining the specific nature and application of those agents which are directly opposed to excess of action, either throughout the whole system, or in particular parts and functions of life.

In alluding to the very powerful medicines of this class, and particularly to the prussic acid, it may be worth while to add one comment; needless, indeed, to all who are familiar with the principles of therapeutics; but requisite to be kept in view while these agents are still waiting, as it were, for recognition, and deprecated by many who see in them only the virulence of their concentrated forms. What we have mainly to regard, in estimating the medicinal value of any substance, or its just application to practice, is the well-defined nature of its action on some organ or function of the living economy. If this action be clearly ascertained, we have essentially a curative power in our hands. Every medicinal agent, even the most simple, is capable of being misused by excess; and this excess, or the fitness of use, is determined, not by any comparison of the power of different agents, but simply by the nature and amount of the effects peculiar to each. The prussic acid, diluted as befits the particular application given to it, is not, in any practical sense, a stronger medicine than others most familiar to us, nor more dangerous in use. And we have even some additional security in the more definite nature of its effects, and in the greater care bestowed on its administration.

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CHAPTER XXXI.

ON THE HYPOTHESIS OF ANIMALCULE LIFE AS A CAUSE OF DISEASE?—ON CHOLERA.

I PUT this title interrogatively, as expressing what is at present merely a speculation; and, in so far, distinct from most of the other topics of this volume; yet sufficiently within the scope of possible or even probable truth, to justify a few remarks upon it. In making these, the reasoning may most conveniently be applied as an argument for the hypothesis; viewing it constantly, however, under the qualification just named.

For reasons, which will be given hereafter, I have taken the Indian cholera as the example best fitted to illustrate the theory before us. Sixteen years have now elapsed since this chapter was first written, during which period the records of this disease have been greatly extended by its repeated recurrence in Europe and other parts of the globe; and volume has rapidly followed volume in description of its progress and phenomena—in speculation as to its causes—or suggestion of methods of prevention and cure. Having admitted that I am defending an hypothesis, in which the proof is one of presumption only, I may perhaps be allowed to add that I find no cause, hitherto, to retract or alter any part of the original argument. On the contrary, I feel justified in considering it as enforced by what has since been added to our knowledge of the phenomena of the disease, and by the failure of other hypotheses proposed. The changes I have thought it well to make amount to little more than a slight extension of the reasoning, and a clearer specification of some points bearing upon the question. In all other respects the argument remains the same as at first proposed.

The question, under its most general form, is—What weight may we attach to the opinion that certain diseases, and especially some of epidemic and contagious kind, are derived from species

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of animalcule life; existing in the atmosphere under particular circumstances; and capable by application to the lining membranes, or other parts, of acting as a virus on the human body? This is by no means a new speculation, and it would seem to have been much more frequently started during the last century than at the present time.[1] The greater exactness of modern inquiry rightly represses all opinions which have not explicit facts for their support. Though the course of discovery has recently been approaching, in some points, nearer to the hypothesis in question, it still furnishes nothing beyond stronger presumptions and more numerous analogies; nor has any endeavor been made to collect or class these, with a view to more general results. Nevertheless the subject is one fairly open to inquiry; and the more reasonably so, as we possess no information regarding the causes of these maladies which can supersede research; but rather have in our ignorance the motive for pursuing it through every new channel which science may disclose.

That there are conditions of animal life in the atmosphere (however characterized), as minute, as numerous, and as variously diffused, as those of which the microscope informs us in water and other media, may be considered from analogy next to certain. Our actual knowledge carries us so far into these minute forms of existence, and by such uniform gradations of change, that we cannot suppose the series to stop, because evidence is no longer drawn from our own senses or means of research. This would imply a sudden breach of continuity, such as we find in no other part of the scale of animal being. It is only of late that the wonderful eye of the microscope has clearly disclosed to us that vast domain of life to which the infusoria belong—a new world of organized and active beings, which, but for the access thus afforded by the happy invention of a single instrument, might have remained forever as much hidden from

[1] Kircher is known as one of the earliest propounders of the opinion. Linnæus gave his sanction to it, by inserting in the Amœnitates Academicæ several memoirs on the subject. The most detailed is that under the name of Nyander, entitled "Exanthemata Viva," in which small-pox, measles, the plague, dysentery, syphilis, and hooping-cough are all attributed to the agency of minute animals, chiefly Acari of different species. A second paper in the same work, on Lepra, applies the speculation also to this disease; and other memoirs, severally entitled "Mundus Invisibilis," "Miracula Insectorum," and "Noxa Insectorum," produce the hypothesis in a more general form.

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our sense and knowledge as the invisible forms of organic life, of which the hypothesis before us presumes the existence. The expression of Locke that "in all the corporeal world we see no chasms or gaps," is a happy anticipation of what modern science is every day more largely disclosing to us.[1]

Whether we may hereafter reach more direct evidence on this subject is still uncertain. As a natural effect of the difficulties of the research in this medium, the atmosphere has not yet yielded to the microscope all that the earth and waters have afforded of these invisible forms of animal life. From direct observation they are removed, unless some such method as that suggested below should be found attainable.[2] Other means however, may be conceived as possible, seeing the number and variety of resources furnished by modern science, and the unexpected quarters from which knowledge is often derived. I may name as an instance of this the paper of Dr. Wollaston (*Phil. Trans. for* 1820), "On Sounds inaudible by certain Ears;" showing the probable existence of whole domains of insect life, capable of exciting vibrations in the air, of which man's grosser hearing is wholly unconscious; but which, received by their finer organs as audible sounds, minister to purposes of enjoyment and activity among beings unperceived by any of the human senses.

[1] Even the unaided eye, indeed, gives us certain probable notices of these swarms of minute beings, which escape all individual examination. Reaumur and other naturalists have conjectured that the small floating clouds (*nubeculæ æthereæ*), like more opaque portions of the atmosphere, which, under summer temperature and in certain lights, are seen near the surface of the earth, are in fact insect swarms, depending for their fugitive existence on the conditions of the medium around them; but in this, as well as in their living habitudes, resembling the insect species more obvious to us. The phrase of Cuvier is in no wise exaggerated, when he speaks of the "*richesse effrayante*" of the insect world. It is an abundance and diffusion of animal life, well fitted to inspire wonder and awe.

[2] It has been supposed that the collection and condensation of dew, in situations where malaria or infectious miasmata abound, might afford a possible means of subjecting these material agents (for such they doubtless are) to chemical or microscopical research. If it were true, as presumed in the hypothesis before us, that minute animalcules are concerned in giving pestiferous quality to a portion of air, might not coloring matter, suitable to animal organization, be applied to the natural or artificial dew condensed from this air, so as to afford the chance of similar success to that which Ehrenberg has attained in his researches on the infusoria? The absence of any such observation, when he was himself seeking to discover infusoria in dew, does not disprove the possibility of this; as, from the difference in the cases presumed, the results, if ever thus obtained, would probably be only partial and occasional.

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Even admitting, however, that we may never reach actual proof of these more minute forms of life, be they insect or of other kind, the probability of their existence is little lessened by the failure, seeing the obstacles which produce it. And if existing, the same analogy, carried further, will lead us to other not less probable inferences regarding the habits and instincts, in which they may be presumed to have affinity with the known insect genera. Such are, their frequent sudden generation, at irregular and often distant periods, under certain circumstances of season or locality, or under other conditions less obvious to apprehension:—and the diffusion of swarms, so generated, and with rapidly repeated propagation, over wide tracts of country, and often following particular lines of movement. The further inference, which more especially concerns our subject, rests also on analogy, though of less explicit kind, viz., that certain of these animalcule species may act as poisons, or causes of disease, upon particular parts of the body exposed to their influence.

These presumptions, supported by various reasons, and contradicted by no ascertained facts, give foundation to the question before us. Whatever is true or peculiar as to the habits of insects, or the forms of animalcule life obvious to our senses, is likely to be equally applicable to those whose minuteness removes them further from our observation. Their generation may be presumed to be even more dependent on casualties of season and place;—their movements determined by causes of which we have less cognizance;—and their power of morbidly affecting the body to be in some proportion to their multitude and minuteness. It cannot be deemed too mechanical an idea, that all conditions which give readier and more extensive access to the internal membranes, to any species capable of acting as a virus on these parts, must greatly increase their influence as causes of disease. It occurs then, as the first point of inquiry, whether we have grounds for believing that animal life under this form can act as a noxious or poisonous influence on the human body. And here there are numerous facts which justify an affirmative answer; as showing that animal matter of certain kinds, applied to absorbing surfaces, may produce the most virulent symptoms of disorder, locally or generally; according to the nature of the virus, its intensity, or the relations of the texture first affected to other parts of the system.

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The evidence on this subject varies in kind and conclusiveness. That which is most familiar and direct is our actual knowledge of numerous animal poisons; the product, as well of living secretions, as of changes taking place in animal matter after death. Respecting the former class, the most important to our present purpose, it is enough to refer generally to their many varieties in all parts of the animal kingdom, and particularly among the insects. From the dangerously active venom of the Argas Persicus and the tarantula, to the more harmless poisons of the bee, the wasp, the ant, and other insects which surround us, we have every gradation and variety of these secretions, which thus act noxiously on certain textures of the human body; and the instances would be much more numerous, if including those which affect other animals.[1] In the Cantharides we have a solitary example of their application to medicinal purposes, and at the same time a remarkable instance of the virulent action of an insect poison on the internal membranes. The class of reptiles affords many well-known instances of such animal poisons, under the form of natural secretions, fulfilling purposes in the economy of the several animals possessing them. The virus in hydrophobia is an example of the poisonous production of disease. Recent observations have shown that the glanderous matter of the horse is capable of producing the same disorder in man; and it is probable that there are many more instances of this kind than we are yet acquainted with. Some of the secre-

[1] In the narrative of the travels of Humboldt, Ehrenberg, and Rose, in Siberia, the last of these eminent naturalists describes a pestilence prevailing on the great steppe between the Irtish and the Ob, affecting very fatally both men and horses; the most obvious symptom that of suppurating tumors, chiefly on the exposed parts of the body. The opinion on the spot is, that the disease proceeds from the stings of insects; and there are various reasons for presuming it to be so. Many similar instances of greater or less authenticity, might be drawn from different sources, attaching disease to the casual or periodical occurrence of this cause.

Speaking on this subject, I would hazard the question, whether the epidemic tendency to carbuncular boils during the last two years (1853 and 1854), so strikingly marked in England, as well as simultaneously in various parts of Europe, has not depended on *causes from without;* and possibly on some virus, or form of organic life, thus acting on the particular textures affected. The singular uniformity of the appearances; their definite course when not interrupted by internal treatment; the manner of their succession; and the seeming absence of connection with any constitutional disorder: these circumstances concurrently would appear to warrant the question I have stated. That other varieties of boil are generated by other causes of local irritation, does not disprove the *possibility* of this view.

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tions from diseased surfaces in the human body are known to acquire properties highly noxious, even to the frame within which they are generated; and the singular effects upon others from inoculation, even with the smallest quantity of such morbid matter, are familiar to every medical man. We may perhaps attribute to the same cause the occasional poisonous quality of certain shell-fish, which commonly are innocuous as articles of food. Or it may be that the latter are instances of a virus produced after death; as we have many examples of animal matter undergoing change of this kind during the process of decomposition, after life is extinct.

If it be urged that none of these or similar instances apply expressly to the question, whether or not minute or invisible animal species may become causes of disease, the answer is, that the nature of the inquiry supposes evidence much less accessible in kind; and that we must seek for that of analogy and presumption where more direct proof cannot be had. Such evidence is found in the facts just stated, and in others still to be mentioned.

One argument for the hypothesis before us, not complete indeed, but having much of reason and analogy to justify it, is founded on our increasing knowledge of the Entozoa; and of the various morbid products, as well as diseased actions, which are due to the presumed propagation of these animals within the body. Without assuming what some have supposed, that tubercles and carcinomatous formations are thence derived, we have sufficient proof through recent discoveries, that these morbid actions from parasitic animals are much more numerous and varied than has heretofore been believed. It seems certain that future research will still further increase the number within our knowledge; and while indicating, by aid of the microscope, forms more minute and actions more definite than those yet discovered, will in these circumstances suggest their more extensive operation as causes of disease.[1]

[1] Almost at the moment of transcribing this (1840) I see the notice of the discovery of minute worms (*polystoma sanguicola*) in the expectorated blood of phthisical cases (Andral, Delle Chiaje, and others had before discovered cephalocysts in the venous blood of man, and even of some of the invertebrate animals); and also of animalcules (*vibriones*) in syphilitic pus. The latter fact, however, appears from one of the papers already cited, in the Amænitates Academicæ, to have been ascertained nearly a century ago. In other morbid secretions other animalcules have been seen; and, according to M. Donné, the author of some of these researches,

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The same remark will apply to the ectozoa, and to the spurious worms infesting the human body as well as many other animals. They afford further proof of the extent and variety of those connections which subsist between different forms of living organization. It is a curious evidence of the progress of such research, that while only eleven species of intestinal worms are recorded in the 12th edition of the Systema Naturæ of Linnæus, nearly 1000 species are described by Rudolfi in his Entozoorium Synopsis, and others have since been discovered.

It is a remark of Ehrenberg, and seemingly founded in reason, that, looking to the extreme fecundity of some of the Entozoa, there is more cause for wonder at the limitation of their effects by the actions of the living bodies they inhabit, than at any morbid effects they appear actually to produce. The speculations of this eminent naturalist derive a sanction for their boldness from his remarkable discovery of the fossil infusoria, as well as from his researches among the entozoa, in their various species, and in different parts of the globe. In ascertaining the highly organized structure, astonishing minuteness, and fecun-

with constant relation to the acid or alkaline nature of the discharge. The occasional presence of a minute worm (*trichinia spiralis*) in the muscular substance, has been ascertained in this country; and still more recently another entozoon (the *cysticercus cellulosa*) has been found by Dr. Knox and others in the same texture. That excellent observer, Professor Owen, to whom we owe the best classification of the human entozoa, has been led by observation to suppose that the cataract of the eye is produced by a species of worm; a supposition at variance, however, with the results to which Sir D. Brewster has been led by his refined and beautiful researches on the crystalline lens in its healthy and diseased states. It is well known that minute animals (*filaria oculi*) have been seen in movement in the aqueous humor of the eye of the horse. Still more recently they have been observed in milk. Different animals, according to Gruithausen, exist in pus and mucus. Some of these statements need confirmation, and, above all, the identity of the animal parasite in similar conditions of disease; but, if obtaining such proof, they manifestly become very important to all future inquiries in pathology.

I have already alluded to the effect of animal poisons *from without*, upon the skin and cellular tissues. The question has further arisen whether any, and in what instances, the common cutaneous diseases may have their origin in parasitic life, animal or vegetable in kind. Omitting the more obvious examples of the Guinea worm, the acarus of Scabies, &c., the question chiefly applies to the different forms of porrigo, impetigo, and herpes. The recent discovery of fungous sporules in certain of these eruptions (as the *tricophyton furfurans* in the ringworm of the scalp), gives a sanction to the inquiry, though not settling it at present by any sufficient proof.

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dity of these animals, he has obtained arguments for the belief that they form a direct cause of many of the diseases affecting man.[1] Reasoning upon the probability that the minute ova of parasitic animals are taken up by the absorbents; and deposited in particular textures of the body, he hazards the opinion that Scrofula, in its various forms, may be thus produced;—a supposition not wholly new, nor incompatible with the fact of its being an hereditary disorder; but certainly requiring more evidence than has yet been given; and perhaps incompatible with the relation of size between the ova of entozoa and the capillaries or lymphatics of the animal textures.

However this may be, the whole subject of parasitic animals and plants, and of the mutual relation of each class, is replete with curious matter of research; and now first pursued with an earnestness proportionate to its importance. It provides us with argument and analogy from every part of organic existence, in attestation of the fact, that the life of one being is in innumerable cases supported by the life of another; and that there are express relations of dependence of this kind established throughout creation, scarcely less definite and remarkable than those by which the functions of individual existence are carried on. These relations—the result of this *inter-penetration* of life in different forms—though often compatible with health, are in other instances the direct cause of the morbid actions and changes which constitute disease.[2] And the conjecture cannot

[1] It must be admitted, however, upon more recent evidence, that some of Ehrenberg's conclusions require material correction; such, for instance, as the complex organization he assigns to the Polygastrica; and the attribution of animal life to many microscopic forms which later observations have shown to be of vegetable nature.

[2] It is a striking proof of the extent of the fact stated above, that entomologists have ascertained more than forty genera of insects to be infested by parasitic worms (*filariæ*), and that it is often possible to determine the species of the insect by that of the parasite living upon it. Even yet more remarkable are the facts recently discovered as to the parasitic growth of minute fungi upon the bodies of different animals; as the Muscardine upon the silk-worm; the fungus which grows vigorously on the Polistes wasp of the West Indies; another cryptogamic plant which has been found even on the body of the common fly, &c.; thus affording the converse of the numerous cases in which animals live upon plants, and completing all the possible mutual relations of parasitic growth in animal and vegetable life. The discovery of *living monades* within the bodies of certain of the human entozoa, as in certain hydatids of the liver, is another circumstance not less worthy of notice.

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be deemed a rash one that time and further research will both add to the number of these instances, and illustrate more clearly the various manner in which such morbid actions may commence, and be carried to maturity.

If admitting, then, that certain diseases are thus produced by one species of animal life (whether truly parasitic or otherwise) acting on another, we must adopt the conclusion, as probable at least, that the symptoms of such disorders are derived, partly from the progressive changes incident to the affecting cause—partly from the vital or other actions of the recipient;—and also that under both these conditions there will be a tendency to regular form, series, and duration of the morbid phenomena. And we are still further carried forward in this argument, to the notion of infection as appertaining to such diseases;—the element of life, thus introduced, affording a more intelligible idea of the generation of fresh virus, and its communication from one individual to another, than any other view with which science or common reasoning has hitherto furnished us.

These considerations, it cannot be denied, are of much interest to the general theory of disease; and the latter, more especially, affords large scope for inquiry. They have close reference to the contagious exanthemata in particular; and though not sanctioned by any direct proofs in relation to these diseases, yet fully justify the prosecution of the research through every possible channel; and render plausible, at least, the arguments of those who have ventured to support the opinion that they depend upon the action and phenomena of parasitic life.[1]

These facts are deeply interesting in themselves; and further remarkable as showing the exactness of modern observation, and the advance it has made into the most secret and minute operations of nature. Ehrenberg has used a bold, but not unjustifiable figure, in speaking of the "*milky way* of the smallest organization"—accessible, we may add, to human research, like that of the heavens, only through artificial instruments; but yielding to these, in the one case as in the other, the most marvellous proofs of the infiniteness of creative power.

[1] I have alluded to this view in a former chapter—"On Diseases commonly occurring once in Life." The most recent and powerful advocate of the doctrine is Dr. Henle of Berlin; in whose Pathological Researches, published this year (1840) there is a very interesting memoir, entitled "Von den Miasmem und Contagien, und von den miasmatisch-contagiösen Krankheiten." In this he maintains the opinion that the material of all contagious disease is not merely organic, but matter possessing all the conditions of parasitic life as regards the bodies affected by it. And in vindicating this doctrine, he applies with great ingenuity the facts recently

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In the foregoing remarks, I have noticed only the more general reasons for supposing it possible that certain diseases to which man is subject, may be derived from animalcule life, existing in the atmosphere around us, and capable of acting noxiously on the human body. To carry the inquiry further, it is necessary to take some instance which may render its details more explicit, and connect together the analogies on which it is founded. Such instance is best sought for in the class of epidemic or contagious diseases; where, if evidence exists at all of the influence of this cause upon the human frame, it is most likely to be found. And amongst the number, there is none to which the hypothesis so well applies, as to the Asiatic or Epidemic Cholera; that strange pestilence of our own time, which, while affrighting every part of the world by its ravages, has seemed to put at nought all speculation as to its causes, or the laws which govern its course; —a disease, nevertheless, which by the mystery of its first appearance—its suddenness, inequality, and fatality—and the failure hitherto of every method of treatment—may well excite the inquiry of all who are zealous for the improvement of medical science.

In taking this remarkable disorder for the instance required, I would merely affirm that it furnishes the best evidence in a case where we can yet attain none other than presumptive proofs. Presenting close analogies to other diseases of this class, its essential characters are so strongly marked, both as to symptoms and manner of diffusion, as to fit it well for being taken as a type of the rest. It must further be admitted that the ground is one singularly free for speculation up to the present moment. I have met with no opinion as to the causes and mode of propagation of the epidemic cholera, which can be considered to afford even plausible explanation of the facts; and in putting to trial the capability of another hypothesis to solve the phenomena of the disease, we are but seeking, by comparison of difficulties, to attain the conclusion which is least liable to them. In truth, the manner in which the assured circumstances in its history annul

ascertained, as to the connection of fermentation with infusorial and vegetable life; and the evidence also lately obtained, regarding infectious disease produced by parasitic influence in certain lower animals, of which that curious disorder, the muscardine of the silkworm, furnishes the most remarkable example.

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all theories, hitherto proposed, as to its origin, has almost the force of an argument for any new opinion or line of research.

The outline of the problem to be solved may be stated thus. Here is a disease, which, appearing first in the Delta of the Ganges, and diffusing itself gradually over the provinces of India, subsequently spread with more rapid course, so as to embrace within a period of seventeen years almost the whole habitable circumference of the globe—reaching China on the one side, the Mississippi and Mexico on the other—its general course traceable, step by step, over the whole of this vast distance, yet very irregular in details both as to time and space—frequently appearing in remote parts long before it affected countries much nearer the general line of its direction; yet never, even in these cases, without traces of its presence and progress throughout various parts of the intermediate distance.[1] During the whole period of its first diffusion, as well as during the twenty years which have since elapsed—under every climate, and in every place of its occurrence—the disease has been absolutely identical in kind; the only variation that of the degree of intensity and virulence. In whatever countries it has existed, a tendency has been observed to its reappearance as an epidemic, in successive years; though often with change in the particular localities affected, and under other conditions still very imperfectly known.

These are the points which it is essential to keep in mind in all reasoning as to the cause of Cholera. Other phenomena of the disease are scarcely less remarkable; but these stand so completely at the threshold of the question, that no opinion on the subject can be true or complete which does not include them, and furnish adequate explanation of their nature. In an argument embracing such a multitude of details, and so much that is still vague to our knowledge, it is important to define these essential points, as well as to remove in the outset all cases and questions which can justly by reasoning be eliminated from the argument.

[1] The Cholera had raged at New Orleans a year or two before it appeared in Sweden, and four years before it devastated Naples and Sicily. But it existed in emigrant ships making the voyage from Liverpool and Greenock to Quebec and New York; and might be traced along the great line of river communication in the United States, as well as by the coast line from the Northern States, to those of the Mexican Gulf.

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Holding these things in view, any hypothesis regarding Cholera may best be prefaced by certain facts of negative kind; the admission of which gives clearer space to the remainder of the inquiry. Of these the first is, that no conditions of physical change in the atmosphere itself are known to us—statical, chemical, electrical, or other—which afford even a plausible explanation of the phenomena of the disease. The earliest Indian official reports furnish nothing that can be admitted as proof to this effect; nor have the later and more exact observations of Europe better warranted the opinion. Some arguments drawn from the prevalence of storms, and seasons of drought or excessive rain, during the years directly preceding its first appearance in Bengal, even if they gave any sort of explanation for India, would in no wise explain the successive spread of the disease over other parts of the globe.[1] Among those who have written on the subject, there are some who have dwelt much on a presumed electrical state of the air as the exciting cause; but without any just or sufficient grounds for the hypothesis.[1] Admitting that there is much still to be learnt regarding this great natural agent, and its relation to the body, nothing certainly has yet been discovered bespeaking its connection with the disease in question; and the whole history of Cholera is utterly at variance with such opinions. Staking the question on a single argument, is it possible that a state of electricity (compatibly with what we know of its laws) can exist in any one spot, capable of destroying life in a few hours in one person, while others in the same place are scarcely affected in their ordinary

[1] Dr. Prout noticed a very small but constant excess in the average weight of the air, during part of the time when the Cholera first prevailed in London; but as there has been no repetition of this remark, either here or elsewhere, it was doubtless a casual coincidence. In reasoning upon questions of this nature, it is especially necessary to guard against conclusions from single and uncompared facts. The very obscurity of the subject tends to accredit hypotheses on evidence far slighter than would be required in other cases; and it is incumbent upon men of science not to take advantage of its facility.

[2] It has been a favorite idea that negative electricity of the atmosphere might favor the production of Cholera; an opinion scarcely professing to have other basis than that the ordinary electricity of the air is positive. M. Peltier, from constant observations during 1835 and 1836, states that in 1835 the clouds were almost always in positive state; in 1836 generally neutral or negative. In neither of these years, except in a few insulated cases, did the disease appear either in France or England.

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sensations? Or, again, that detached portions of air, thus strangely peculiar in their electrical state, should move irregularly (yet ever tending to follow the tracks of man) over continents and across oceans, still retaining their virulent power of producing disease? Such notions stand in contradiction to all we know of the laws and conditions of electrical action on the globe. They are generally propounded by those who are content to wrap ignorance in the garb of scientific language, without due knowledge of the particular physical agency they invoke, or of the kind of evidence by which alone such questions can be rightly decided.

All the arguments used in a former chapter, to prove that the Epidemic Influenzas are in no way directly produced by atmospheric states or changes, apply with equal or greater force to the disease before us. No notion of an epidemic constitution (to acquiesce for a moment in a phrase thus vague and doubtful in import) can be of avail against the facts belonging to the history of the disorder—its course, manner and time of progress, and various aspects—as it has spread over the world during the period, now nearly forty years, since the occurrence of the earliest known cases at Jessore. Had I been writing a treatise on Cholera, instead of a few remarks on a particular hypothesis, I might bring the most ample evidence for the truth of this negative assertion. The whole resolves itself into this:—that the disease in its most distinct and virulent form, has existed in different places under every possible variety of atmospheric state;—and, conversely, that every such variation has existed in higher degree in the same places, and at all times, without producing the disease.[1] The admission of these facts makes it almost needless to enforce further the conclusion from them.

[1] I have myself, at different periods, been in five several places where the Cholera was prevailing at the time; and have had sufficient proof of the different atmospheric circumstances under which it may subsist. Even with the disposition ever present to find especial causes for remarkable events, the records of former pestilences are equally wanting in admissible evidence as to any states of atmosphere or season likely to produce them. Professor Hecker, indeed, in his History of the Epidemics of the Middle Ages (the Black Death, the Sweating Sickness, &c.), has sought to connect them with contemporaneous records of earthquakes, eruptions, floods, droughts, &c.; but a close examination of his argument will show how loosely his conclusions hang upon the premises. Searching through the world for them, similar and equally authentic catalogues of natural phenomena might be obtained for almost any period, without any corresponding association of pestilential disease.

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Nor have we the smallest reason, from knowledge or analogy, to assume that any gaseous, mineral, or vegetable matter, diffused in the atmosphere, or exhaling from the earth, could create a disorder thus peculiar, or spread it in a manner so remarkable over the face of the globe. The notion of terrestrial or mineral exhalations—a favorite one in all ages, and espoused by Sydenham—is defective in proof in every case, and singularly inapplicable to the cause and circumstances of Cholera. A natural morbid cause, or causes (for which, in default of a better, we must admit the name of *malaria*) may originate locally, and produce various local endemic or epidemic diseases; and of this we have sufficient evidence. But these very circumstances of limitation directly exclude any agent, so generated, as the source of a migrating disease, to which we are unable to assign boundary or limit.

Equally inapplicable, for the same reasons, is every theory founded on the temperament, habits, food, or other conditions of particular communities. The history of Cholera, as followed through different countries, and climates, and races of mankind, negatives at once all suppositions of this nature; nor need we follow them beyond the mere statement. Whatever be discovered hereafter as the cause of the disease, it must be one which has come into existence and active operation within the last thirty-eight years; and which, therefore, cannot possibly depend upon conditions long before existing without the production of any such effects.

Dismissing, then, as improbable, or even as impossible, these different views as to the origin of Cholera, the inquiry forces itself upon us, whether any other hypothesis be more compatible with the strange phenomena of this disease? To any one approaching the inquiry, after having compassed the negative conclusions just stated, the words of Cicero may well occur: "*Utinam tam facile vera invenire possem, quàm falsa convincere.*" It is, indeed, in this case far easier to convict of error, than to discover and to prove the truth.

With this admission, which must needfully be made, let us come to the question proposed in this chapter,—whether the hypothesis be tenable, which looks to animalcule life, diffused by the atmosphere or by man, as the source of the disease?—a

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form of life not cognizable by our senses, or other present means of research; but nevertheless subject to the same general laws of propagation and diffusion as species more obvious to us; guided by special instincts like every other part of the animal creation; and producing a virus which acts noxiously or fatally on the body of man.

In pursuing this subject, we must take account severally of the more essential facts regarding Cholera, to see how far they admit of such explanation; premising what will now be generally allowed, that the disease, though resembling the common Cholera Morbus in some of its symptoms, is actually distinct from it, as it is from every other disorder of which we have previous knowledge.[1]

The first condition is the obvious and assured one, that the cause of the disease must be a *material poison*, definite in its nature, and specific in its effects. We cannot reason on the subject without this admission; nor, under the shelter of vague terms, apply to the phenomena any hypothesis which excludes it. It is true that some of the suppositions already noticed admit a virus or specific cause; and use may be made of the analogies furnished by other disorders, avowedly infectious through matters generated within the body. But this is at best analogy of difficulty rather than elucidation; and carries us little way towards solution of the circumstances which form the peculiar history of Cholera. It must be kept in mind that we have to deal here with a migrating malaria—a wandering cause of disease;—capable, not merely of being diffused through the atmosphere, and conveyed along vast tracts or lines over the globe, affecting different places with a varying intensity, which no known conditions of earth, atmosphere, or human habits, will explain; but also possessing the power of reproducing itself, so as to spread the disorder by fresh creation of the virus which first evolved it.

This *faculty of reproduction* (admitting the term for what we

[1] This is a point where we may recognize the influence of names in opposition to the most obvious facts. The Asiatic Cholera, certainly unknown to us before, reached Europe from the spot where its origin is dated, by steps, every one of which is known and recorded. The resemblance in certain of its symptoms to another more familiar disorder is but what occurs in many other relations of disease, and does by no means prove identity of nature or cause.

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cannot well otherwise express) stands foremost among the conditions essential to a right theory of Cholera. All our reasoning stops short, unless under recognition of the fact so stated. Without it there would seem a physical impossibility of explaining the phenomena of the disease; and particularly its distribution and succession in different places and seasons. A thorough study of these singular details of its history, keeping this principle constantly in view, will not only confirm and illustrate the latter, but lead us to organic life, as the only conceivable source and subject of such reproduction. It is against all the analogy of nature to suppose this power to belong to inorganic matter. Either animal or vegetable life, in their simple forms, must furnish the material cause we seek for; since to them alone can belong the faculty of renewing unlimitedly the active cause of the disease. Assuming the former, we must attribute the reproduction of the virus to some animal process; occurring either within the bodies of those affected, and acting therefore by infection; or produced by animal changes and propagation taking place without. Through this hypothesis we obtain a conception of the reproduction and diffusion of the natural cause of the disease; curiously conforming, as we shall see, with our observations of its phenomena, and also with what we know of the instincts and habits of insect life.

Here, however, a question arises *in limine*, whether our argument may not be satisfied by the assumption of vegetable life, as the cause sought for; without going to a higher grade of organisms, in quest of the virus of the disease? Such question is not only reasonable in itself, but justified by the parity of many of the arguments which apply to the two hypotheses. In all that concerns the great distinction just laid down, between organic and inorganic existences, they mainly coincide; while the difficulties incumbent upon both may be considered essentially the same. Such analogy might well be expected; seeing how closely or almost inseparably, animal and vegetable life coalesce, under their simplest and most elementary forms. Modern science has been perplexed in its endeavor to find terms for a definition, which may clearly and naturally distinguish two modes of existence, thus nearly approaching in their lower organisms, while so widely diverging from each other in the higher parts of the scale.

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I must add, further, in relation to this question, that the hypothesis of the origin of Cholera from the diffusion of fungi, or kindred forms of cryptogamic life, capable of acting as a poison upon man, has been recently proposed, and ably argued, by more than one naturalist.[1] The argument, as far as it is special, is chiefly founded on the facts, that certain fungi have been discovered parasitic in the human body; that many species are noxious in their effects on animal life; that almost all have enormous powers of rapid reproduction; and that their presence in the atmosphere is attested, not only by the part they bear in many phenomena of fermentation and organic decay, but also by sudden and extraordinary deposits of mould, of various kind and color, upon different bodies on the earth's surface. The records of every time and country tell of those *showers of blood*, or *blood rain;* once deemed the omen of impending evil, but now known to be fungous deposits of this vivid red color;—whence derived, or why thus suddenly appearing, nothing in our knowledge enables us yet to say. Two singular instances of this kind, mentioned in the note below, have occurred in places where Cholera prevailed at the time.[2] They might, upon that frequent

[1] I would mention especially a valuable memoir by Professor Daubeny, "On the Influence of the Lower Vegetable Organisms in the Production of Epidemic Diseases," read to the Ashmolean Society of Oxford, November, 1854; and since reprinted in the Edinburgh Philosophical Journal.

[2] These instances occurred in the two distant localities of Philadelphia and Berlin. During the presence of the Cholera in Philadelphia in 1832, there was seen on one occasion the strange phenomenon of a mould of bright vermilion color, suddenly tainting the bread and other articles of farinaceous food, in various parts of the city. Precisely the same thing happened at Berlin on the 26th of October, 1848, the Cholera prevailing there at the time. I received from Professor Ehrenberg some specimens of the bread thus singularly tainted; the microscopic examination of which showed (as it had before done to him) the presence of two fungi, and one animal organism, in this film of bright red mould. There are several instances on record of similar sudden falls of these colored moulds in places where epidemics existed at the time, as at Cremona in 1529; but, as I have stated above, this is a matter in which the negative evidence far outweighs the affirmative.

We find it related by Quintus Curtius that at the commencement of the siege of Tyre by Alexander, a fall of blood occurred; terrifying those both within the city and without. This story, unsupported as it is by the authority of Plutarch or Arrian, would hardly deserve mention but for the curious coincidence which enters into the narrative, of the *bread* having been thus affected. "*Apud Macedonas quoque, quùm forte panem quidam militum frangerent manantis sanguinis guttas notaverunt.*"

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mode of interpretation, which raises mere concurrence to the higher relation of cause and effect, be deemed an evidence in the question before us. But any such inference is at once annulled by the innumerable local examples of Cholera without similar concurrence. And even were it otherwise, the evidence would be rendered ambiguous by the detection of animal organisms, associated, as it would seem, with all such fungous deposits; showing the wonderful interblending and coalition of these microscopic forms of life; and leaving it doubtful (as in the instances of periodical blight, or mildew, on plants of human culture), to which we must assign the earlier place in the phenomenon.

While giving all due weight to the facts just stated, and believing that were the animal hypothesis of Cholera discarded, none other than that of a vegetable virus could take its place, I still am of opinion that the former is more in accordance with the phenomena of the disease. We cannot deny the possibility that fungous sporules may be transported to great distances by the atmosphere; retaining any specific virulent properties they possess, and the power of reproduction to unlimited extent. But the peculiar manner of progress and distribution of Cholera, and the course and character it assumes in any given locality, though not easily submitted to any certain law, are nevertheless, in many ways, too definite to admit of being explained by such vague diffusion of the elements of disease as would belong to vegetable life. We can hardly look elsewhere than to animal instincts (more various and wonderful in aspect, as we become more cognizant of them) for solution of these and other problems, so essential to any just theory of the disease.

I do not further discuss here the question between the two hypotheses, because the examination of the several phenomena of Cholera, in argument for one of them, will best aid us in making the comparison. To this examination I proceed; after having clearly, as I hope, shown the necessity of admitting a material virus of the disease, and its powers of reproduction, as the foundation of inquiry; thereby excluding all vague hypotheses as to atmospheric or telluric causes independent of organic life; and reducing the question to the simplest form of which it is capable.

The *manner of diffusion* of Cholera (taking this phrase in its

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largest sense) will be easily apprehended as the important point of the argument; comprising, as it does, all the most essential questions; and defining the facts and analogies necessary to their solution. I have already alluded to the first great spread of the disease, from the place of its origin, across continents and oceans, in a continuity of course, so as well-nigh to have compassed the whole northern hemisphere of the globe. Looking now to the more special measures of its diffusion, one of the most striking facts we find in the history of Cholera, is its frequent suddenness of appearance and disappearance in given localities; while, in other places of its occurrence, a regular period and series of events are observed, marking what may perhaps be called the *normal course* of the epidemic. The former case is exemplified to us in the sudden invasion of insulated towns or villages, apart from all obvious communication;—the disease disappearing again, almost as suddenly, after having inflicted a certain number of deaths, generally beyond the average proportion of mortality in this disorder. In the second and more common manifestation of Cholera, as it occurs in large and crowded cities, a few detached cases are first observed, also for the most part fatal in event;—then, after a certain period not very distinctly marked, a wide and fearful prevalence of the disease usually for two or three weeks;—and, again, a period of gradual decline, during which the cases are less malignant; and the recoveries become frequent in proportion. Taking the whole course of the disorder in any one place, the rate is singularly uniform between the number of those affected with the true Cholera collapse and the amount of mortality; thus further showing how definite and uniform is the action of the virus, amidst all the inequalities of its local distribution.

The numerous volumes written upon Cholera, describing its progress through different countries, abound in curious examples of the facts just stated; as well as of others, more anomalous, which have endlessly perplexed the question as to the contagiousness of the disorder. Yet these conditions, seemingly so incongruous, accord well with what we know of the habitudes of animal life, under the forms here presumed. The sudden appearance and wonderfully rapid multiplication of insect swarms, at certain times, and in situations favorable to their propagation, are facts familiar to naturalists. The diffusion of

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such swarms—sometimes by continuous flight for great distances; sometimes by tardy, broken, and divided progress—is equally familiar to observation; however scanty our knowledge of the peculiar animal instincts by which those movements are governed.[1] The same analogy extends to the disappearance of these swarms;—either suddenly, by further flight, or atmospheric changes on the spot;—or more gradually, by completion of their term of existence; want of food; or any physical causes tending to destroy their ova, or retard their development and the successive metamorphoses they undergo.

In various respects, indeed, the erratic and ambiguous course of Cholera is well represented by the flight, settlement, and propagation of the insect swarms which inflict blight upon vegetable life. Their appearance at different and often distant periods, without obvious cause for such irregularity;—their direction to certain plants only;—their settlement upon these in clusters and detached localities;—the frequent suddenness of their change of place and final disappearance;—are all circumstances of remarkable analogy; as also the curiously abrupt limitation of some of these swarms, showing itself in definite lines of direction, along which their work of destruction is carried on. These lines, as well as the other *local configurations* of vegetable blight, though to our observation seemingly arbitrary, are doubtless connected with the instincts of life under this form, and with the relation of such instincts to the surrounding media. The diseases, thus produced in certain trees at particular periods (the elm, the plane-tree, &c., are recent examples), furnish many curious proofs as to the erratic influences of parasitic life; and still more are obtained from plants of artificial culture. The blighted potato or turnip field, or the vineyard, are pages on which to read the his-

[1] The Hessian fly, on its first appearance in America, afforded a singular example of this slow progressive movement. First observed in Long Island in 1776, it proceeded into the interior, at the rate of ten or fifteen miles in the year, destroying all the wheat in its progress. The appearance and spread of the *Blatta orientalis* in the northern parts of Europe is another fact of similar kind.

We are more familiar with the history—wonderful and incomprehensible though it be—of those vast swarms of insect life which suddenly appear at undefined periods of time, sweeping over wide tracts of country, and marking their course by the devastation they inflict. The locust swarms and migrations furnish the most remarkable example of this phenomenon. In a succeeding note I shall have occasion to mention a curious analogous occurrence, of recent date, in our own vicinity.

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tory of many natural phenomena; and the philosopher may often best go to the farmer or gardener for knowledge of those details, which no theory can disregard.

Applying the facts thus obtained to the case of Cholera, we find striking analogies to multiply around us; offering presumption at least, where no certain proof can be had. This disease has been often defined in its course by similar lines of direction; sometimes stretching over considerable tracts of land or sea; but more frequently obvious in particular localities; where the partial distribution of the virus, in lines, or over limited *patches* of surface, is often attested in very curious ways. In neither case can explanation be obtained from any physical conditions of the globe; from currents of air; or local circumstances of soil and climate. Nor will human communication, though certainly concerned in part in the transmission of the disorder, resolve these phenomena; of which innumerable examples might be cited, from the first appearance of Cholera in India to its latest ravages in Europe. We have similar occurrences in the history of some other epidemics; but none perhaps so remarkable in kind and degree.

Connected with these facts is the observation, seemingly well attested, that the Cholera sometimes spreads in face of a prevailing wind, and where no obvious human communication is present;—a circumstance difficult, if indeed possible, to be explained, without recourse to animal life as the cause of the phenomenon. No mere inorganic matter could be so transferred; nor is vegetable life better provided with means for overcoming this obstacle.

Another analogy, as it may be considered, to the habits of insect life, is the apparent preference of direction along the course of waters, which has been noticed in the spread of the disease. This is one of the few facts which seems to have acquired something like certainty. Nearly every other conclusion as to what may be termed the *natural history* of Cholera, has been curiously refuted by fresh observations, almost before usurping the name of discovery. And even here, though the evidence is stronger and more complete, yet is it still not free from the contradictions and anomalies which press on every part of the subject. No certainty is yet attained as to the manner in which water—running or stagnant, pure or impure—favors, or other-

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wise affects, the diffusion of the disease. We cannot say more than that, under admission of the general fact, the animal hypothesis accords with it better than any other.[1]

Again, the frequent occurrence of insulated cases of Cholera in any given locality, before the more violent outbreak of the disorder there, though in some instances it might be referred to infection, cannot be so explained in others. The fact, however, has close analogy to what is observed as the habits of swarms of aphides, and other insects, infesting plants; and it would be difficult to find any more plausible solution than one which connects the phenomenon with animal life.

The reappearance of the disease in the same countries or localities at uncertain intervals, but generally during the hotter part of the year, is another fact bearing on our hypothesis. The earlier evidence on the subject left much that was obscure as to the causes concerned in these recurrences of Cholera; and even now we can but vaguely conjecture what may be due to fresh virus, brought (as originally) from remote distances,—what, to the revival of the germs of former disease on the spot itself. Observations indeed have been now endlessly multiplied; but on this, as on all other points, with such strange diversity or contrariety of results, as rather to perplex than settle the question. The vast collection of facts obtained by the Cholera Committee of the College of Physicians (admirably arranged, and commented upon by Dr. Baly and Gull in the published volume of Reports), goes chiefly to show the present impossibility of assigning laws, either to the original diffusion of the disease, or to its recurrence in particular localities.

Difficult of certain solution on any hypothesis, these phenomena

[1] If I were to specify any instance within my own knowledge, peculiarly illustrating our ignorance of the causes which give local direction and malignity to Cholera, it would be that of its occurrence in 1835 at Trollhättan in Sweden; which place I visited immediately after the cessation of the disease. It is a village of scattered, clean, and comfortable dwellings; standing on primitive slate rock just above the highest of the great cataracts of the Gotha, the waters of which, emerging from Lake Wenern, are as clear as those of a mountain torrent;—the only accumulation of refuse about the place the saw-dust from the saw-mills on the river. In this village, so circumstanced, the Cholera suddenly appearing, in a very short time carried off forty-eight people—about one-tenth of the whole population of the place. The mortality at Gottenberg at the same period was nearly in the same ratio; but here there are local conditions which (granting the interpretation usually put upon them) might better explain the result.

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are less so, I think, on the supposition of an animal virus than on any other. This theory offers the contingencies of new swarms arriving;—or of the development of ova or other germs of existence, deposited during the preceding invasion of the disease, and called more or less numerously into life, by increased temperature or other physical causes;—or perhaps of the concurrence of these conditions in extending or otherwise modifying their effect.[1] Such views sufficiently accord with the facts ascertained regarding Cholera, and are reconcilable with all we know of insect life and reproduction. The connection of each great outbreak of the disease in Europe, with the warmth of summer and autumn, and its general suppression in winter, may indeed furnish argument either to the animal or fungoid hypothesis; but more especially, I think, to the former. Animal ova, or germs, do often remain dormant for long and indefinite periods of time; yet like the seeds of plants, retain the powers of life, and burst into active existence when circumstances occur to favor the change. One of the most singular facts in natural history is this sudden appearance of insect swarms, with all their instincts complete, in localities where they have not been seen for years before; either brought from unknown distance, or the produce of the ova of former seasons deposited on the spot, in preparation for this later development.[2]

[1] A valuable document on this subject is the paper by Dr. Budd and Mr. Busk, in the 21st vol. of the Med. Chirurg. Transactions, on twenty cases of Cholera, which occurred in October, 1837, in the Hospital Ship at Greenwich. The circumstances of limitation to this vessel, in which the Cholera had twice occurred before, gave strong proof of a local reproduction of the virus of the disease. Such special instances often do more to elicit truth than a multitude of widely spread but detached observations.

[2] Though such instances are familiar, even in our own country, I may briefly notice one, somewhat remarkable, and of recent occurrence. In October, 1836, a vast swarm of minute aphides (whether one of the numerous known species was not ascertained) passed over a wide district in Cheshire, Derbyshire, and the southern parts of Lancashire and Yorkshire. The air was so thickly filled with them that the clothes and faces of persons walking out of doors were completely covered. When getting into the eyes they excited considerable inflammation. The height to which the column reached could not be known. From the best observations in one locality, its superficial extent must have been at least twelve miles in one direction by five miles in another; but the detached notices from other places make it certain that the continuous swarm was much more widely spread. No sufficient comparison appears to have been made of local observations to furnish proof as to the rate and direction of movement; but it is worthy of notice, that

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And here we again approach to speculations, which, though founded on the most minute forms of existence, have yet a vastness in their obscurity, and in the results to which their solution would lead. I allude to the hypothesis of equivocal generation; —the question, whether animal or vegetable life (for the inquiry equally regards both) is in any case produced without the egg, or seed, of prior individuals of the species? The result of modern research has been chiefly on the negative side of the question; and the old dogma of "*Omne vivum ab ovo*" remains sanctioned to an extent, which considering the exquisite minuteness of the objects, could scarcely have been supposed within the scope of human observation. Where, as in the genus Aphis, eight successive generations of females may be fertile without fresh impregnation, or where such phenomena exist as have been described under the name of Parthenogenesis and Metagenesis—it seems needless to seek for other manner of production. Nevertheless, we are still very far from certainty. Instances may be indefinitely multiplied to the effect just stated, and yet it is possible that there are also simple germs of life around us, awaiting development; or matter so constituted as to be capable of assuming new and various forms of living organization, according to the circumstances present to favor such change. The generation of some of the Infusoria would scarcely seem explicable on any other view.

The question, thus obscure to our present knowledge, is closely connected with that, equally obscure, as to the immutability of species. In both, the greater familiarity of the idea of successive propagation by ova, or by seed, disguises the fact that such propagation is quite as incomprehensible in itself, as the conversion of the same organic elements, under altered circumstances, into new forms. In the second Chapter of this volume (on Hereditary Disease), I have stated my belief that all present evidence tends to establish the opinion of the permanence of species. And this belief may be held, even while fully recognizing the tendency and result of recent inquiry to introduce

the town of Manchester was infested by these insects for two or three successive days. Wherever generated, or by whatsoever instinct carried on, there is cause to suppose that the swarm was in transit from one place to another, and possibly brought nearer to the earth by some peculiar state of atmosphere existing at the time.

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life as an element and agent, both in past and existing phenomena, where the relations of inert matter only were before presumed to exist. We may never reach the entire solution of this question; but time and research will undoubtedly bring us nearer to it hereafter.[1]

Without entering further on these controversies, we find in facts, well ascertained, of the occasional length of time during which the ova of animal life retain their power of reproduction, much that bears on the hypothesis before us. There is reason to presume, upon various evidence, that the simpler and more minute the form of organization, the greater is the faculty of thus retaining life in a dormant state. And if complete animal organization, such as that of the infusoria, or the vibriones of wheat, is capable of being restored to vital activity, after long apparent extinction, still more may animal ova be supposed retentive of the principle of life and reproduction, awaiting merely the conditions necessary to their evolution.[2] These considerations bring us to one of the most curious questions respecting Cholera, viz., how a poison should be thus locally generated, unknown before, but capable, by some means of reproduction,

[1] Notwithstanding this imperfection of our knowledge, the whole subject of organization, whether of animal or vegetable life, affords a signal testimony to the progress of modern science;—realizing on the one hand some of the poetical imaginations of antiquity;—on the other, by mathematical exactness of observation directed to the most minute forms of matter, laying a basis for discovery, of which the boldest speculator can scarcely yet see the full extent. We may agree with Malebranché,—"Il est bon de comprendre qu'il y a des choses qui sont absolument incomprehensibles"—and yet be sedulous in seeing that the line of demarcation is not drawn too closely around us.

The Memoir of MM. Beauperthuy and Roseville, and the observations of Schwann, Liebig, Cagniard de la Tour, &c., are among the most valuable of the recent inquiries bearing on this subject; indicating the development of animal and vegetable life in different media, as the probable cause of the various processes of fermentation, putrefaction, &c. It is one of those curious questions where doubt exists as to the respective conditions,—which is *cause*, which *effect*, in their mutual relation? Such doubt is generally solved in the event by some simple and single observation, deciding not merely the particular problem, but opening a way to knowledge beyond.

[2] In a recent communication of Ehrenberg to the Academy of Sciences at Berlin (April 16, 1855), he relates his observations on the revival, by immersion in water, of microscopic animals (the *Callidina scarlatina*, *Milnesium alpigenum*, &c.) taken up originally from a dry earth, at the height of more than 10,000 feet on Monte Rosa, and preserved for four years, in moss earth, in his writing-desk.

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of being diffused over every part of the earth? I have just alluded to certain obscure points of philosophy—equivocal generation, and the permanence or mutability of species—which bear closely on this problem. Its solution is difficult on any hypothesis; but least so, I am led to believe, on that of animal origin. This furnishes a reason why the disease, existing perchance at some anterior time, may have disappeared for ages; the seed of the pestilence, however, yet remaining, to be called into activity by future contingencies. Or, if we admit the doubtful assertion of some authors, that the same disorder has existed repeatedly in India, though never with equal virulence, no view will so well suggest an explanation as that which admits on the one side all the instincts of animalcule life; on the other, the varying casualties of place, season, and human communities. Swarms may be evolved in a given locality, and perish, before migration or communication to man have given cause to their wider diffusion.

It is here that the argument comes in closest relation to those singular questions regarding certain other diseases, such as Small-pox and Syphilis; which have perplexed all medical reasoning and research, and still remain unsettled and obscure. The presumption as to these diseases is, that they were unknown to the ancient physicians;—that they have appeared in Europe (if not in the world) at some later but uncertain date;—and that they have since undergone modifications of character, either from human precautions, or under that mysterious law (if we are entitled to call it such) by which the physical constitution of races and communities of men seems in time to *habituate itself* to certain morbid agents and conditions of disease. This latter expression may seem vague; but language is forced to become so, when touching upon subjects of this nature.

Another, and perhaps closer, relation may also come into notice here; associating Cholera with those other strange and fearful pestilences which are recorded in the history of every age and country—desolating great cities, or diffusing death more widely over whole nations. The plague of Athens has gained celebrity from the place and period of its occurrence, and from the genius which has been given to its description. The plague called the "Black Death," as it occurred in Florence, has received similar celebrity from the pen of Boccaccio; but these

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partial records give faint idea of such a pestilence as that last named, which, about the middle of the 14th century, spread into every corner of Europe; producing a mortality beside which the history of Cholera is a fair page to look upon.[1] The "Sweating Sickness," as manifested in its different irruptions upon England and all Europe, from 1485 to 1550, is another example of these extraordinary wandering pestilences; affording in its history (as does also the Black Death) so many striking analogies to that of Cholera, as regards the manner of diffusion and other incidents of the disease, that it is impossible not to suppose some connection of physical origin. The virus of the diseases is doubtless not identical; but where the effects are thus far alike, the causes cannot be without some certain common relation. I am unable to pursue this topic here, but it well merits further inquiry.

The whole subject of the migration, presumed in our hypothesis, will occur as one of its great difficulties; seeing that we have no evidence of any of similar extent in the animal kingdom; and that the manner and duration of insect life are in many respects opposed to one, which involves such variety of country and climate; most of the species known to us appearing to be submitted to geographical distribution, like plants, and the other classes of the animal kingdom. What may be said in reply (even admitting the restriction to insect life, properly so termed) is, that we are singularly ignorant of all that relates to animal migrations in general;—that our knowledge becomes less as we descend to the more minute forms of life;—that the facts known respecting the occurrence of insect swarms meet some of these objections;—that different insect species vary infinitely in their habits and capacity for diffusion;—that the ova themselves may be widely disseminated, independently of the living animal migrations;—and that the rapidity of propagation and change, incident generally to this part of the animal creation, if intro-

[1] In his treatise on this fatal epidemic (*Schwarze Tod*) Hecker calculates that twenty-five millions of people may have perished in Europe alone. But statistical reports were not then invented; and the sources of his calculation are so vague, and the original details so obviously exaggerated, that it is impossible to assent to this belief. Numbers, ever the subject of popular error, are not likely to have been very exactly dealt with at a time of so great calamity.

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ducing some difficulties, provides the means of obviating others which press upon the argument.[1]

Pursuing the hypothesis (still tentatively upon all these points), we reach the question as to the infectious nature of Cholera; and may gain from hence some further argument in removal of the objections just stated. If this point, so warmly contested, be answered in the affirmative, man becomes an agent in the diffusion; and the transit over continents and oceans, otherwise impossible, is brought within comprehension and the analogies of other disease. Much of this controversy might, as I think, have been obviated by a regard to the principles mentioned in a former chapter. It seems certain that the virus of Cholera, whatsoever its nature, may be spread in different ways. That the atmosphere, though not in any of its states the cause of the disease, forms one medium for diffusing it—and this over great distances—is clearly to be inferred from numerous facts in its history. From the same evidence, and particularly from the details of its introduction into new localities, and passage across seas, it is impossible not to admit that its transference is frequently effected by human means; and it may perhaps be affirmed (without involving the question in more doubtful phrases) that man, the peculiar recipient of this cause of disease, is also the principal agent in its diffusion. On a question of this nature, it is always well, if we can obtain them, to select a few proofs unequivocal and complete, wherewithal to meet the many ambiguous or anomalous cases which are certain to occur: and it would be easy to state several such circumstances in the history of Cholera, scarcely admitting of other interpretation than that just given.[2]

[1] Professor Lacordaire of Liège, one of the most recent and able writers on the geographical distribution of insects, records numerous examples showing the influence of particular physical causes upon their distribution or casual diffusion; while yet leaving many instances of diversity and anomaly to which no known explanation will apply.

[2] One, which I think includes every condition of perfect evidence, is the transportation of Cholera from the infected towns of Liverpool, Dublin, and Greenock to Quebec, in vessels having emigrants on board, among whom the disease existed during the whole passage across the Atlantic. The details of this occurrence, including the place, time, and manner in which the Cholera first showed itself in Quebec, are such as to leave no point of ambiguity. I have before stated the fact that the disease appeared here, at New York, and other parts of the United States, long before it occurred in many parts of Europe, out of the main line of its west-

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Allowing this, some of the apparent contradictions which have embarrassed the question as to contagion may be solved, by referring to the habitudes of that class of animal life which the hypothesis assumes; and particularly to the circumstances which favor the propagation, diffuse the ova, direct the flight, fix the resting-places, or modify the virus, of the swarms thus supposed. We can understand in this sense the respective relations of man and the atmosphere to the spread of the disease. The human body may be a means of concentrating the morbid cause; and conveying it, possibly in a state more prone to inflict the disorder, to distant and detached localities. The atmosphere (the medium in every case of communication from one body to another) may itself, independently of man, carry the agent of disease over wide distances; and by its changes in temperature, humidity, and electrical state, produce many of the modifications observed. And in the separate, or concurrent, agency of these causes of communication, a plausible solution is found of most of the singular anomalies presented to us.[1]

Some allusion may be expected here to the question of Quarantine;—whether of avail, or otherwise, in preventing the ingress of Cholera into any particular country or locality? The views, and entire course of argument, pursued in this chapter, will show my opinion to be, that it is wholly ineffectual; and this judgment I consider to be amply confirmed by the evidence of facts. The practical conclusion is one of great importance; but it will be long, I fear, before we can expect it to be generally adopted and acted upon.

That man should be so peculiarly the subject of Cholera is a difficulty which belongs to all hypothesis, but least perhaps to

ward course. The circumstances which attend the transmission of the disorder over land are not often so explicit in their conclusions.

The Treatise on Pestilential Cholera, by Dr. Copland, published in 1832, contains an excellent statement of the argument regarding contagion, as of all besides relating to the disease. In adopting the opinion of its infectious nature, he has ably and justly dwelt on the inadequacy of the experiments by inoculation, and certain other modes of exposure, to negative this opinion.

[1] Some physicians have urged the opinion that the virus of Cholera is conveyed through drinking water, or food, which has been contaminated by the evacuations, or other contact with patients under the disease. Were this proved to be so, the fact would readily connect itself with our hypothesis; but it is too doubtful to be admitted as an argument.

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that of an animal origin. It is difficult to conceive any other physical cause thus far exclusive in its effects; while, in support of this view, we have various analogies in insect life; and, generally, in the nature and habits of parasitic life, as already noticed. Though man, however, is chiefly obnoxious to this virus, whatever it may be, we have some proof that he is not exclusively so; and it is worthy of note, that the most distinct evidence relates to its effects on birds; and to the rare appearance of certain species of birds in the localities where Cholera is prevailing at the time.[1] The cause assigned in the hypothesis under review is at least as probable as any other in explanation of this curious fact.

The question, connected with the preceding, why the miasma of the Cholera should destroy some persons, and leave others in the close vicinity little if at all affected, applies equally to other disorders of contagious and epidemic kind, and presents similar difficulties to every hypothesis. These difficulties, however, attach themselves much more forcibly to the notion of an inorganic agent, than to a supposition which includes all the conditions of animal life; and particularly of that form of life which is so rapid and abundant in reproduction, undergoes such remarkable changes, and is submitted to instincts of which we have so little cognizance. No view, as far as can be seen, better illustrates the various degrees of the disorder;—from the diarrhœa or ill-defined disturbance about the epigastrium and bowels, subsiding without further issue—to the powerful and virulent disease, producing instant collapse, and destroying life within a few hours of the earliest seizure. The fact that every gradation of disease exists between these extremes is scarcely compatible with the idea of any simple physical agent; but accords far better with the notion of an animal virus, variously diffused in a given locality, and as variously acting upon those exposed to its influence.

[1] This statement of the effect upon birds is fully confirmed in a late report of the Medical Faculty of Vienna, upon whom the inquiry as to the influence of Cholera upon other animals was expressly enjoined. A similar fact has been noticed in the history of other pestilences of older date, and attributed vaguely to atmospheric changes affecting this class of animals. The report just named is the most complete document we possess on the subject, and contains many very curious observations.

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These considerations bring us more directly to the pathology of the disease, and the relation of the various alleged causes to its actual symptoms. The weak part of medical science lies here, where morbid agents from without come into contact with living actions in the body: and if we fairly examine into this part of pathology, we shall find how little certain knowledge has been gained from the earliest date of medical history to the present day. Singular though the symptoms of Cholera are, in their suddenness and fatality, they offer no difficulty which does not equally belong to other kindred diseases. We may even go a step further, and affirm that the notion of an animal virus, applied to absorbing surfaces, and engendering the disorder by entering into the circulation, is that which on the whole best accords with the character of the disease, and with the analogies most obvious to other morbid affections. We have many proofs of the power and virulence of different poisons of this class, and of the remarkable changes they produce on the nervous system and the blood;—often so speedily after their absorption, that all observation is frustrated in seeking to follow the train of events; and speculation equally at fault in attempting to find a theory for them.

The action of the morbid cause in Cholera seems to have most kindred with these poisons; the change which takes place with such rapidity in the properties of the blood being, as I think, the great feature in the disease; the basis probably of all the other symptoms. The extreme depression of animal heat throughout the body;—the peculiar secretions so largely poured out from the inner surface of the intestines;—the suppression of the natural secretions;—the severe spasmodic actions;—the various effects on the nervous system;—and the typhoid and other symptoms which occur in sequel to the acute stage of the disease;—are all more readily and consistently explained, by looking to this altered state of the blood, as the effect first resulting from the influence of the morbid cause.[1] Some of these symp-

[1] I would apply the same remark to the diseased or disorganized state of the inner coat of the bloodvessels, observed in many of the dissections after death from Cholera, and particularly described by Dr. Mackintosh.

The experiments of Dr. Namias, of Venice, are alleged to show that inoculation with the blood from cholera patients, under collapse, will often destroy other animals, as rabbits, within a few days; and by changes which render their blood

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toms, indeed, scarcely show themselves where the Cholera exists in so virulent a form as to terminate life in a few hours; proving thereby that they are not essential to the character of the disorder, nor to its event. This fact (which I believe to be a well-attested one) helps us here, as in many other medical questions, towards a right theory, by distinguishing the secondary and subordinate symptoms from the primary and essential conditions of the disease.

It is conceivable indeed, and we have no direct proof to the contrary, that the original impulse of the virus of Cholera may be upon some part of the nervous system. But I consider the supposition much more probable which makes the influence on the blood the first in the series of changes: and it is undoubtedly that which has most obvious and general relation to the other symptoms of the disease. If the magnitude of the effect here seems out of proportion to the cause, we have but to look to the certain influence of other morbid agents on the blood;—whether the imperceptible virus of many contagious diseases—the minute quantity of certain other animal poisons producing death—or the action of different salts in preventing the coagulation of this fluid. Inability to explain the rationale of such changes applies alike to every view of the subject; and cannot therefore be admitted as an exclusive objection to one alone.

In the subject of this Chapter there is nothing which directly involves the inquiry as to the prevention or treatment of Cholera. Nevertheless the interest of these questions is such, and their relation so intimate with every hypothesis as to the origin of the disease, that it is impossible to exclude them from consideration. Neither the hypothesis of which we are treating, nor any other alleged cause, can be considered as hitherto affording even plausible suggestions to this effect. The requisitions in common to every view are, the discovery of means to prevent the access of the cause—or a specific capable of obviating the virus when received—or remedies adequate to sustaining the body under its influence,—objects, it must be confessed, hitherto wholly unattained. The singular uniformity in the proportion of deaths, in every place of the occurrence of Cholera over the globe, not

again destructive to others of the same species. I doubt, however, if these experiments have been authenticated by that repetition which is so essential in every inquiry of this nature.

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only affords proof that no valid means of cure have been discovered for the disease, when fully developed in its more malignant form; but shows, moreover, how inert even the most drastic remedies become, when directed against this extraordinary poison. Methods of treatment the most opposite, yet equally insisted upon by their respective advocates, and many of them such that their use would be dangerous in other cases, lose here all their distinctive effects, and are found alike inefficient and harmless. I have seen from 100 to 150 grains of calomel given within twenty-four hours; not merely with no abatement of the disease, but with scarcely a single obvious effect on the character and course of the symptoms. Opium and brandy, and the many other powerful astringents and stimulants which have been brought into trial, fail of all their wonted effects; or act so ambiguously, that the most sanguine practitioners can hardly record them as remedies for the disease.

I doubt not that in some habits, and especially when the morbid cause is present in slight degree (a contingency ever to be kept in mind), good may be done by all means which tend to moderate the symptoms, restore right secretions, and sustain the powers of life. But until the proportion of mortality under collapse is distinctly abated by some given plan of treatment; and this effect well attested by comparison with other places where different methods, or *no methods at all*, have been pursued; the inevitable conclusion will remain, that no real antidote has yet been discovered; and that the cases of true malignant Cholera, which we interpret as cures, are rescued from fatal result by causes which we can neither recognize nor command.

This may possibly appear to some too disparaging a view of what has been attained in the treatment of Cholera. It will not, I think, be felt as such by those who duly examine their own experience, and all that has been derived from the most authentic records of the disease. These records, as already remarked, are now exceedingly numerous; drawn from every country, climate, locality, and condition of social life; and so tabulated in many instances as to convey very exact averages. The results thus obtained, if fairly weighed, admit but of one interpretation. They show us that zeal, ingenuity, and science have been taxed in vain to discover a remedy, effectually and uniformly capable

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of lessening the proportion of deaths, among those attacked with the disease under its more severe forms. I say *effectually and uniformly*, because some of these tables seem to yield results partially favorable to certain methods of treatment. But the difference of proportion obtained is so small, and the conclusions require to be qualified by so many doubtful circumstances, that it argues rather a sanguine spirit than a sound reason to be satisfied of their stability. Where Cholera reaches the state of collapse, our practice is still that of experiment only; and of experiment little guided by any principle or uniformity.[1]

If the disease should still continue in the world, and a remedy, justly so called, be happily discovered hereafter, it will probably be of the nature of a specific antidote to the action of the virus on the blood. I hazard this opinion, looking to the singular importance of this action in the pathology of Cholera; to the promise which animal chemistry gives of new methods of altering the state of the blood; and to the actual results obtained by means expressly applied to the object in question. I allude here to the treatment by saline injections into the veins; which, though not yet sufficiently established by experience, and liable to various difficulties in its use, has nevertheless in many cases

[1] I allude here chiefly to the report of the Cholera Treatment Committee of the Board of Health. The Committee took cases, under collapse from the disease, as the foundation of their inquiry; and reasonably so, since in no other way could they have obtained even an approach to parity of trials. Slight or incipient cases of the disorder are obviously incapable of furnishing any scale of symptoms explicit enough for comparison. Allowing for difference of results from the treatment within hospitals or elsewhere (a manifest source of ambiguity in many ways) the inference from these tables would seem to be that the treatment by *Calomel and opium*, and by *Salines*, tends most to lessen the mortality under collapse. But we are bound to remark that this inference is drawn merely from the comparison with other modes of treatment (by stimulants, astringents, emetics, acids, castor oil, &c.), which, though propounded by medical authority, would appear on the evidence of these tables to be positively injurious in effect.

It is an ancient medical maxim, and of high authority, "*ωφελειν η μη βλαπτειν.*" A question may be started, with which this maxim has close concern, whether it is possible to reach any sound conclusion without including in the averages those cases where there has been no treatment at all. If the tables show a mortality of 75 per cent. under one method of treatment, and only 60 per cent. under another, this does not logically prove the latter to be beneficial, but merely gives evidence that the former is injurious. The only valid comparison is that to which I have just referred. And to this we must look for whatever certain conclusions regarding the treatment of Cholera we are likely hereafter to obtain.

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been found to possess singular power in checking the progress of collapse; and approaches nearer, perhaps, to a successful issue than any other method yet employed. Still it is only an approach; and the problem yet remains to be overcome; either by accident, or the better fortune of future experiment. Casualty may perchance serve us here better than reason; but we are bound to follow the latter as far as it will lead us.

The best contingency, next to that of a specific remedy, seems to be the discovery of means capable of sustaining or restoring the nervous power of the vital organs, so rapidly and deeply affected by the disease. Any agent equal to fulfil such purpose must presumably be both simple and powerful:—whether Electricity, in any form of its action, may be capable of this, is a question not yet fully answered. Speaking generally of the treatment of Cholera, I certainly do not believe that an efficient method of cure will ever be attained by dealing with the symptoms in detail. The magnitude of the disease, and intensity of its cause, are beyond such means of reaching them; and it is probably only in the milder cases—where the virus has been slightly received, and diarrhœa is the single or principal symptom—that any favorable result can thus be obtained.

After all, is it just to speak of our ignorance of a remedy for Cholera as a peculiarity of this disorder? Have we any more certain or specific cure for other contagious or epidemic diseases unless dignifying with this name that mitigation of symptoms, and prevention of particular mischiefs, which alone come within the rule of safe practice in most of these cases? The instance is one among many, where long familiarity with certain disorders, or their milder character, disguise the connection they have with other diseases, less known and of greater virulence. The preventive remedy discovered for Small-pox is an eminent, though partial, exception to the remark; and an indication, at the same time of what may yet be possible in other diseases where a specific virus is present—the cause of malady to the individual, and the means of its dissemination to others. Whatever the nature of the virus of Cholera, the contingency of discovery is the same here as elsewhere: and if, like the small-pox, it is to be considered a new form of disease engendered in the world, and destined to remain as a wandering pestilence here, the attain-

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ment of this object is manifestly most important to human welfare.

These remarks, however, have no peculiar application to the hypothesis before us. This has now been discussed in as much detail as a speculation merits to be in which direct proof is wanting; and for which a main argument is the inadequacy of all other hypotheses to explain the facts. I have not noticed the further objection which may be urged against it, viz., that, if well-founded, its application would not be limited to Cholera; but must of necessity extend to certain other contagious or epidemic diseases (and especially to the epidemic influenzas), of which the essential conditions are so far the same, as to warrant the belief of a similar, though not identical, cause. In the justness of this inference, I must acquiesce; and admit also the extent to which it carries speculation in advance of our actual knowledge. This in truth is the wider question, serving as title to the present Chapter;—how far, and in what manner, the animal life of one species may become a source of disease to another? And I selected the instance of Cholera, simply as furnishing the most various and remarkable illustration of the argument.

The general question is one largely open to research;—sanctioned in its object by our ignorance of other causes for these genera of disease;—affording some curious presumptions where certainty may be unattainable;—and involving a topic than which none is more remarkable, viz., the origin and progress of those disorders, which, if not newly generated, have first appeared in our parts of the world within the period of modern record; and in some cases become endemic, where there is no authentic trace left by history of their prior existence.[1]

[1] While this sheet is passing through the press (August, 1855), the Report of the Medical Council of the General Board of Health, on the Epidemic Cholera of 1853–4 in London, has been presented to Parliament. Among other valuable averages and conclusions which it contains, may be mentioned those as to the proportion of mortality at different ages;—those as to the probabilities of recovery at different stages of the disease;—and those which attest a diminished rate of mortality in some proportion to the elevation above the Thames.

It will be found, I think, that these conclusions (admitting, as is probable, their confirmation by future experience), are in nowise incompatible with the views proposed in the foregoing Chapter.

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CONCLUDING REMARKS.

In the preface to this volume I have briefly stated the motives which originally led me to this manner of publication, and the principles by which I have guided myself in it. But as prefaces are often not read, or scantily remembered, I am desirous to add a few concluding pages, which may form a sort of summary to the foregoing chapters, both as respects their general design and the method of its fulfilment. Such revision, while sought for partly on my own behalf, will not, I trust, be wholly indifferent or useless to the reader.

An object which I have sedulously kept in view in the selection, as well as treatment, of the topics included in this volume, has been their relation to those more general principles of theory and practice, which alone can give sanction to the name of Medical Philosophy. Even in the few instances where treating of particular diseases or remedies, I have sought, as far as possible, to bring them into connection with these general views; believing that in this manner I might render more useful the notes and reflections, derived from a long practice, which I have ventured to make the foundation of my work. The reader will, I think, see that this object has been always before me; and that it has led in several cases to the association of certain diseases, or morbid states, not usually classed together in our systems of nosology. The relations, thus indicated, must ever be important; even if in part superseded hereafter by the discovery of others, still closer and more determinate. In a field wide as that of Medical Science, there are many paths, seemingly parallel in the outset, but ultimately converging towards those higher truths and more general laws, which, here as elsewhere, must be regarded as the just object and end of all research.

I trust it will be further seen, that I have constantly endeavored to separate what is actual and assured knowledge from that

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which but usurps the name and show of it. The nature of the subject peculiarly demands this precaution. The inherent difficulties of Medicine, as a science, of which I have spoken elsewhere, are greatly augmented by its necessary connection with those feelings, fashions, and prejudices of the world, which tend to impair even the soundest judgments, and to mislead them in the search after truth.

In seeking to follow out the principles just stated, I have found it needful to deviate frequently from the usual mode of treating these topics; and in several instances to modify, or impugn, opinions, which have become traditionally current in medical literature. I can affirm that I have never done this wantonly, or otherwise than from conviction. It may, and probably will, be thought by many, that I have taken too disparaging a view of the actual state of medical knowledge, and often inculcated doubts without providing a solution for them. Persuaded of the truth of the maxim, that "it is generally shorter and easier to proceed from ignorance to knowledge, than from error," I have never scrupled to note what I think doubtful or deficient in evidence; suggesting other conclusions where I could venture to do so; and where not, seeking to indicate the channels through which the inquiry might best be pursued in the future. In giving up all false or untenable pretensions, the best preparation is made for remedying what is vague or deficient in our present knowledge.

In the treatment of these various topics, I have further kept constantly in view all that I felt as likely to serve the interests of those entering the profession, or still young in its practice. Some chapters will be noticed, as especially directed to this object. In all I have sought to avoid mere technicalities; and to place in the clearest form those principles of thought, observation, and conduct, which may most conduce to the progress of Medicine, and the honor and usefulness of those who profess it. The experience derived from nearly forty years of active practice may perhaps be admitted as giving some title to offer such advice. A professional life, thus prolonged, must have been unprofitably used, if not affording the suggestion of many things to be learnt or done—of many others to be put aside or avoided. Of these things, thus taught by experience, some will be seen to relate to the moral discipline of the profession (a matter to which

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it is impossible to attach too great weight); others again involve certain technical duties of the physician, not equally essential, yet nevertheless very important to the credit and integrity of practice. The inculcation of these things may lessen at least some of the difficulties inseparable from early medical life, and which now and then blight it in the very beginning.

It will probably be remarked that I have in this volume referred, more frequently than is usual, to the medical authors of antiquity, as well as to those of the age more immediately preceding our own. Had my limits allowed of it, I should gladly have carried these references yet further. The vast aids which Medicine, both in its theory and practice, has recently derived from its growing connection with the other physical sciences, have naturally withdrawn attention from authors who were scantily, if at all, provided with these great resources. But, considering how much the practical part of medicine must ever depend on observation and experience, it is injurious, as well as unjust, to neglect the records of what has been attained by the industry and acuteness of older observers. Those who are familiar with the pages of Boerhaave, Morgagni, Sydenham, and even of the physicians of antiquity, will find in them many of the very cases, methods, and opinions, which are unwittingly offered to us as novelties in the medical writings of our own day.

I must not unduly prolong what may seem a superfluous recurrence to the objects and methods pursued in this volume. But where the topics are so various, and to all appearance desultory, I feel that it cannot be amiss thus shortly to have indicated the principles which have mainly guided me in their discussion. And I the rather venture on these concluding remarks, inasmuch as they will serve to my vindication in certain points, where I may seem to deviate from common opinion, or to carry too much distrust into matters of common belief.

I cannot, however, close what I am now writing, without some reference to another object, of which I have never lost sight in the consideration of these subjects. I mean, the relation of Medicine, both as a science and system of practice, to the other physical sciences—a connection ever tending to closer approximation; and through which its progress to higher attainments and powers is mainly to be looked for in the future. This is a

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field for exertion so vast and various, that every physician may fulfil some part in the required labor, by observation, experiment, or reasoning. Chemistry in its every branch (but especially the Chemistry of Organic Life, associated, as it now is, with the doctrine of definite atomic proportions) stands foremost in the connection thus established. But Electricity, Optics, Meteorology, Natural History, and even the mechanical sciences, severally contribute towards this great circle of relations; which, while unceasingly enlarged by new discoveries, is at the same time ever concentrating itself around common laws and higher generalizations. To these I have often had occasion to refer in the preceding chapters; and have indicated, wherever able to do so, the means by which their association with medical knowledge may best be enlarged and defined.

For from the growth of the other physical sciences, Medicine has not only enlarged its own sphere, and gained a sounder philosophy, but it has also acquired new methods and instruments with which to conduct future inquiry. Numerous and exact records now give effect to that happy method of averages, which we have noted as being an almost mathematical guide to truth. Every well-attested case thus acquires its appropriate place and value; and facts, before lost or dispersed, are now virtually registered in the conclusions to which they have contributed. Of the direct instruments which science has furnished in aid of medicine and physiology, the Microscope is the most consummate and wonderful example. But other refined applications of physical agents have become familiar to our use; giving the power of looking more deeply into the interior of the living frame; of better determining its morbid conditions; and of applying remedies more closely and effectually for their relief.

I must yet advert to another branch of knowledge, now forming an integral part of Human Physiology; the growth and better definition of which may be said to exercise a new influence on every department of medical science. In another volume I have treated of this subject, under the title of Mental Physiology; using the term to express more especially the reciprocal action and relations of mental and bodily phenomena, as they exist in, and make up the totality of, human life. The history of the mind, regarded simply as an inquiry into the faculties and feelings, intellectual and moral, is an inheritance to

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us from early antiquity; and we may doubt whether later research, though using new nomenclature and better definitions, has really added the knowledge of a single phenomenon to those already recorded ages ago. But between these purely mental inquiries, and those which regard the physical nature of man, there lies an *interspace;* destined ever to continue such; yet greatly narrowed, we may affirm, by the remarkable discoveries regarding the functions of the nervous system, and their relation to mental as well as bodily phenomena, which of late years have given a new character to physiology. While recognizing still a line and limit, impassable by human reason or research, we have approached nearer to it on this side; and are justified in believing that the same investigation, further pursued, will bring us yet closer to the boundary. But all experience teaches that it is a ground to be trodden over with caution;—that the evidence as to facts becomes more precarious at every step we advance;—and that ambiguities of language and vague speculation are ever tending to mislead the judgment, and obscure the truth. A sound spirit of philosophical induction, so important in every part of science, is especially needed in this branch of human physiology.

To remove error—to perfect what is incomplete in knowledge (*τα ἡμιεργα ες τελος εξεργαζεσθαι*)—and to attain by discovery what is new—may fitly be placed before the young medical man, as objects not merely compatible with the practical duties of his profession, but as even aiding their more entire fulfilment. Few indeed possess the means requisite to command fame or success in such pursuits. But the objects themselves need not, and ought not, to be alien to any condition of medical life; and the endeavor to attain them, to whatever extent it may in each case be possible, will ever form the best guarantee for the just and honorable discharge of professional duty.

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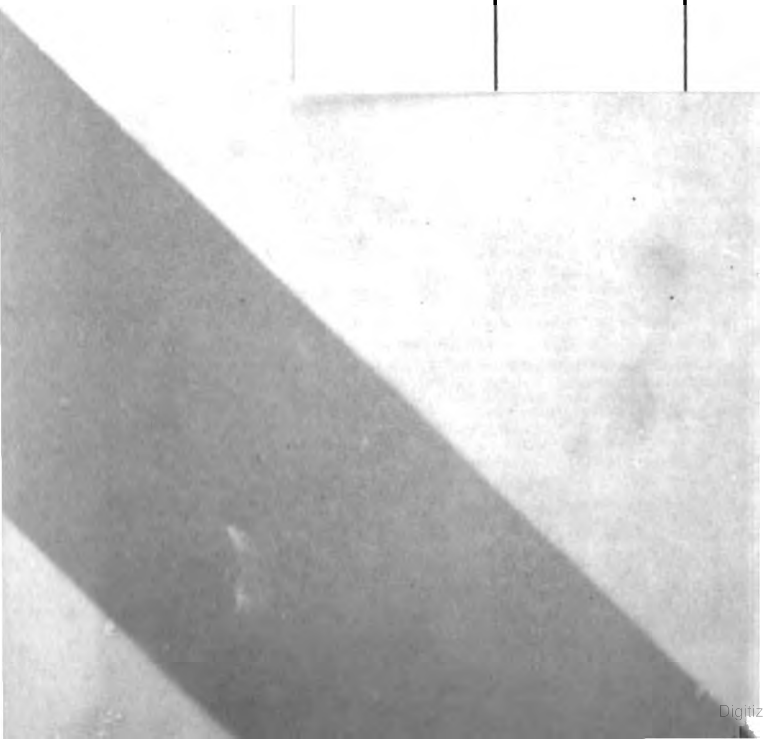

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